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Bulletin 56
(Part 3 of 3 Parts)

THE SHOCK AND VIBRATION BULLETIN

Part 3
Invited Papers, Pyrotechnic Shock,
Pyrotechnic Shock Workshop

AUGUST 1986

A Publication of
THE SHOCK AND VIBRATION
INFORMATION CENTER
Naval Research Laboratory, Washington, D.C.

Office of
The Under Secretary of Defense
for Research and Engineering

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The 56th Symposium on Shock and Vibration was held in Monterey, California, October 22-24 1985. The Naval Postgraduate School and the Defense Language Institute were the hosts.

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CONTENTS

PAPERS APPEARING IN PART 3

Invited Papers — Plenary A

Partial contents:

Pyrotechnic Shock

Pyrotechnic Shock Workshop

PAPERS APPEARING IN PART I

Welcome

Invited Papers

Shipboard Shock

Blast and Ground Shock

Shock Testing and Analysis

PAPER APPEARING IN PART 2

SESSION CHAIRMEN AND COCHAIRMEN

56th Shock and Vibration Symposium
October 22-24, 1985, Monterey, CA

Date	Session Title	Chairmen	CoChairmen
Tuesday 22 October, A.M.	Opening Session	Dr. Young S. Shin, Naval Postgraduate School, Monterey, CA	Dr. J. Gordan Shovalter Shock and Vibration Information Center, Naval Research Laboratory, Washington, DC
Tuesday 22 October, P.M.	Pyrotechnic Shock/ Shipboard Shock	Mr. Henry N. Luhrs, TRW Electronics Systems, Redondo Beach, CA	Dr. John DeRuntz, Lockheed Palo Alto Research Laboratory, Palo Alto, CA
Tuesday 22 October, P.M.	Blast and Ground Shock	Mr. James D. Cooper, Defense Nuclear Agency, Washington, DC	Dr. Anatole Longinow, Wiss, Janney, Elstner Associates, Inc. Northbrook, IL
Wednesday 23 October, A.M.	Plenary A	Dr. J. Gordan Showalter, Shock and Vibration Information Center, Naval Research Laboratory Washington, DC	
Wednesday 23 October, A.M.	Pyroshock Workshop, Session I, Data Interpretation, Design and Test Requirements	Mr. Daniel Van Ert, The Aerospace Corporation, El Segundo, CA	Mr. Henry N. Luhrs, TRW Electronics Systems, Redondo Beach, CA
Wednesday 23 October, A.M.	Modal Test and Analysis	Dr. Robert Coppolino, MacNeal Schwendler Corporation, Los Angeles, CA	Mr. Strether Smith, Lockheed Palo Alto Research Laboratory, Palo Alto, CA
Wednesday 23 October, A.M.	Testing Techniques	Mr. Steven Tanner, Naval Weapons Center, China Lake, CA	Mr. Peter Bouclin, Naval Weapons Center, China Lake, CA

Wednesday 23 October, P.M.	Pyroshock Workshop, Session II, Instrumentation, Data Acquisition, and Data Bank	Mr. Glen Wasz, TRW, San Bernardino, CA	Mr. Paul Strauss, Rocketdyne, Canoga Park, CA
Wednesday 23 October, P.M.	Pyroshock Workshop, Session III, Simulation and Testing	Mr. Dan Powers, McDonnell Douglas Astronautics, Huntington Beach, CA	Mr. Robert E. Morse, TRW, Redondo Beach, CA
Wednesday 23 October, P.M.	Machinery Dynamics	Mrs. Milda Z. Tamulionis, Vibration Institute, Clarendon Hills, IL	Mr. Robert L. Leon, Liberty Technology Center, Inc., Conshohocken, PA
Wednesday 23 October, P.M.	Isolation and Damping	Dr. Bhakta B. Rath, Naval Research Laboratory, Washington, DC	Mr. Ahid Nashif, Anatrol Corporation, Cincinnati, OH
Thursday 24 October, A.M.	Plenary B	Mr. Rudolph H. Volin, P.E., Shock and Vibration Information Center, Naval Research Laboratory, Washington, DC	
Thursday 24 October, A.M.	Structural Dynamics	Dr. John L. Gubser, McDonnell Douglas Astronautics Company, St. Louis, MO	Mr. David W. Gross, RCA Astroelectronics, Princeton, NJ
Thursday 24 October, A.M.	Fatigue, Acoustics and Fluid Flow	Mr. Leonard L. Shaw, Air Force Wright Aeronautical Laboratories, Wright-Patterson Air Force Base, OH	
Thursday 24 October, P.M.	Shock Testing and Analysis	Mr. John D. Favour, Boeing Aerospace Company, Seattle, WA	Mr. William J. Kacena, Martin Marietta Denver Aerospace, Denver, CO
Thursday 24 October, P.M.	Short Discussion Topics	Mr. Howard Camp, U.S. Army Electronics Research and Development Command, Ft. Monmouth, NJ	Mr. Tommy Dobson, 6585 Test Group, Holloman Air Force Base, NM

INVITED PAPERS-PLENARY A

Pyrotechnic Shock

Dr. Sheldon Rubin
The Aerospace Corporation
Los Angeles CA

I am very pleased to be here today because pyrotechnic shock has been an interest of mine for many years. It is particularly satisfying to me that the interest has been sustained and is continually increasing. Pyrotechnic shock is a very lively subject. If you look over the program at this meeting, you will see many papers addressing one aspect or another. There were a number of papers yesterday, there is the workshop today, and there will also be a few papers tomorrow on this topic.

I would like to, first of all, give you a little bit of history as to how this workshop came about. The format of this workshop is a little different from anything that has been done before at these symposia, and we are all anxious to find out how it will work out. In May of 1985, a tutorial on pyrotechnic shock was held on the day after the annual meeting of the Institute of Environmental Sciences at Las Vegas. That tutorial was very successful. I was amazed at the attentiveness and the amount of discussion during that day. We had perhaps 50 or 60 people sitting at tables not too far from a casino, and everybody paid attention and enthusiastically participated in that session. As 5:30 P. M. rolled around, we had to think about giving the room back to the hotel to set it up for that evening's function. People just kept on talking, so the chairman had to actually cut off the discussion.

At the very beginning of the tutorial, Chuck Moening, who was its chairman, asked several questions to try to get some idea of the experience of the audience on the subject of pyrotechnic shock. The design of the tutorial had been premised on talking to novices in a teaching mode. To get some understanding of the real situation, one of the questions was: Who in here has had less than 5 years experience in the subject of pyrotechnic shock? About 10 percent of the people raised their hands. So, the people attending were generally quite experienced. The thought that occurred to me afterward was that tutorials are not probably what are needed, as much as a workshop where communication could take place between experienced practitioners in this field. And so, the idea for today's workshop was born.

When that idea was presented to the Shock and Vibration Information Center, it met with a very positive response. The next step was to create a questionnaire which we sent to the participants of the IES tutorial and some others. Questions were asked like: Would you participate in a workshop? Would you be willing to make a presentation at a workshop? The responses to those questions were also very favorable, and so we have a workshop today. That tutorial, in May 1985, was actually a repeat of a very similar tutorial given three years ago, and sponsored by the Orange County, California chapter of the Institute of Environmental Sciences. In effect, today's workshop is the third in a series begun by the Institute of Environmental Sciences leading to today's workshop. All of those workshops were very well attended.

I mentioned to you that I have been involved with pyrotechnic shock off and on for a number of years. I started working at the Aerospace Corporation 23 years ago. My very first assignment, I think the second day I got there, was to go back East and discuss a test in which a space vehicle segment was cut loose circumferentially from structure simulating the prior stage, with a flexible linear shaped charge. A number of electronic "boxes" were near the separation plane, and there resulted a lot of chattering and transfer of relays. Some accelerometers were on the test article, and there was grappling with how to quantitatively describe the shock event. A typical result was 1500 g's, 0.2 millisecond. This referred to the fact that the time history contained a tallest pulse with a 1500 g peak acceleration, and that pulse's duration was 0.2 millisecond. The questions that I was supposed to address were: Were those data real? Are the thousands of g's really real? What other things might happen to electronic equipment other than relay chatter? What kinds of things should we be concerned

about in these "boxes?" How do we interpret these data? Especially, how do we qualify our system for this type of shock event? And finally, when problems arise such as the relay problem, what do we do about them? How do we correct any problems that show up? Interestingly, many of those same questions are still with us, and they will be addressed in today's workshop.

Shortly after that experience, it was clear that pyrotechnic shock was a serious problem that had to be addressed. My own prior experience had not involved pyrotechnic shock. To my knowledge, this problem was not known to the various spacecraft progams that were ongoing at that time. My first big chore after this was to create a briefing for management, project managers, and up to the Vice-Presidents of the Corporation, to tell them what this pyrotechnic shock problem was all about. Several hundred people were in the audience, and most of them had backgrounds in electronics. Here I was trying to explain what a shock response spectrum was, why this was the most sensible way to describe this kind of event, and that it should be used as a basis for creating test specifications. I don't know how successful I was, because even to people with backgrounds in dynamics, the description of what a shock response spectrum is, and what it means, is not always clear. But apparently I succeeded in at least identifying this as an important problem, and one that had to be addressed. My next assignment was to make sure all the then current spacecraft programs were made aware of the problem and given some information as to the kind of test programs that might be required, and other necessary considerations.

Because of my background with the shock response spectrum, it was very natural for me to recommend its use. As a graduate student, I had worked in an earthquake laboratory and the so-called Earthquake Spectrum, which is basically the shock response spectrum applied to earthquakes motions, was the standard technique for describing shocks for earthquake structural engineering. I had worked in this lab, analyzing data, converting it to shock spectra, and I had also done some research along this line. I had written in the Shock and Vibration Handbook on interpretation of shock records, so it was easy for me to see the value of the shock response spectrum. After all of these years, and despite the legitimate concerns that can be raised about the description of a shock event in terms of a shock response spectrum, I still think it is the most meaningful descriptor. There are some other aspects one has to deal with, but this is the basic descriptor that makes the most sense to meet the need qualifying equipment.

In the intervening 23 years, two Military Standards have been created which specifically address the subject of pyrotechnic shock. Many of you are familiar with MIL-STD-1540, "Test Requirements for Space Vehicles." That was the first one that dealt specifically with the subject of pyrotechnic shock. Just two years ago, revision D of MIL-STD-810 was created, and for the first time, it specifically addressed the pyrotechnic shock environment. That standard addresses environmental guidelines and test methods. I was involved with the language that appears in both of those standards. There is considerable similarity, of course, between those documents even though MIL-STD-810 is more of a methods document and MIL-STD-1540 is not.

One of the considerations that came up very early for us in specifying a shock response spectrum was that it left open the possibility of incorrect application. Given a particular value of Q, and we had selected a Q of ten somewhat arbitrarily, one could perform a sinusoidal sweep to meet a shock response spectrum or any other time variation you could imagine. This was not really the intent. The easiest way to prevent stretching out of the excitation was to put a limit on the duration of the shock event; we came up with 20 milliseconds as the maximum duration for the event as a constraint, in addition to meeting a shock response spectrum requirement. I think this has worked out pretty well. During the rewrite of MIL-STD-810, which went on for several years while a number of drafts were sent out for comment, I was frankly surprised that I never heard from the organizations who provide analysis equipment for the shock response spectra or who provide test control equipment. They didn't seem to think they would get bitten by this; they should have been very interested in the language that was going into that standard because the equipment that they will provide in the future to analyze the test shocks, and perhaps to control the test, will have to meet certain requirements. I am not sure all of that language has sunk in. I have heard things like, "I don't know what MIL-STD-810D is because it has never been a requirement in any of our programs." It's coming, so I recommend to those of you who are interested in pyrotechnic shock programs, even if it is not a requirement in your present program, to look over the pyrotechnic shock portion MIL-STD-810D and at least become aware of its approach. It represents the recommended test method for all three of the armed services. If you take issue with what is in that document, it would be advisable to carefully prepare your arguments in advance and to propose constructive alternatives.

I am leaving most of the time for Chuck Moening because I hope that his talk will stimulate you to believe this pyrotechnic shock problem is really important. I am not sure everybody considers it to be so. I think there are good reasons for many not to be aware of what can go wrong and what has been the experiences operationally. Chuck will now address that subject and I hope will enlighten you.

Views of the World of Pyrotechnic Shock

Charles Moening
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Los Angeles, CA

I want to say a few words about my perspective, or my window regarding "Views of the World of Pyrotechnic Shock". As Sheldon mentioned, I have been in the aerospace industry for some 27 years. The data I am about to show you, come from a number of people who have worked in the aerospace industry for a long time. The implication is that my window presents a perspective from the aerospace industry. It's important to understand that point of reference because if you are working in the shipboard area, or maybe in the aircraft area, or other areas not mentioned, the window you are looking through is probably different from mine. The reason I say this is, with aerospace vehicles, basically there is one chance to succeed for any given vehicle. It must fly completely successfully the first time, or else loss of a complete mission may be the result.

I want to talk about the lessons learned in these 27 years of experience. I would like to present this experience in several different formats. One format that I will start with, to keep it a little light, is what I call "Famous Last Words". Following this I want to ask a few questions, self-examination questions of how we in the Shock and Vibration Community have performed. I will answer those questions by reviewing flight failure data, and then we can draw conclusions from that. I will close by asking the question, do we need to do anything different? Do we need to improve our success or has it been adequate? Can we continue doing what we have been doing in the past? I will not limit this strictly to shock. I would like also to address vibration to the extent that flight failures have occured due to vibration.

Let us consider the first "Famous Last Words" in Figure 1. These were essentially my baptism to the pyro-shock world. We were examining shock data from a separation test that involved the separation of missile stages by firing explosive bolts. We had data that showed extremely high acceleration levels, but very short durations. The question was, do we really need to be concerned with that environment? The situation at that time was our equipment had already been qualified to shock levels that were typically used at the time, 100 g's, 6 milliseconds, which came from a MIL-STD. Vibration tests had been run on the hardware, and in those days, the missile equipment vibration specs were very severe, in the 50 to 100 g RMS range. The assumption made was that the high amplitude, short duration shock environment was probably not significant compared with the vibration test. That proved to be a poor assumption, as indicated by the bottom of Figure 1 which reflects subsequent flight experience. About a year later, during launch of one of the ICBM's, a relay, which was located near the launch release bolts, and which was a part of the range safety destruct system for the missile, was transferred closed; the relay fired the destruct system, and it caused a true pyrotechnic event over the whole launch pad. That essentially led me to begin accumulating a body of data which eventually resulted in the publication of a paper in the 1984 Aerospace Testing Seminar.

Figure 2 shows the time frame, the circa 1960-1970, after it was recognized that pyrotechnic shock actually was an environment to be concerned with. At the time the industry was going from the time domain to the shock response spectrum domain to define shock environments. We were faced with levels of about 6 to 8 or maybe 10,000 g's, and when such test levels were given to an electronics equipment designer who was familiar with g's in terms of static acceleration, his reaction was: "My equipment! There is no chance it will pass." The experience, as shown at the bottom of Figure 2, has been that most, but not all, off-the-shelf avionics equipment can withstand levels of several thousand g's. I will discuss this more a little later when I review the types of failures which have occurred in flight.

Figure 3 shows another problem. This is a problem that arises when a program already has its equipment qualified. A system test is

conducted, shock levels are measured, and it is found that shock levels are above what equipment has been qualified to. Now there are two options: (1) Equipment can be requalified or redesigned which can be very expensive because there may be many suppliers whose contracts must be renegotiated, and possibly hardware that needs to be redesigned, and, (2) the second and easiest solution is to reduce the shock. That may solve the problem. The experience part of Figure 3 indicates that the second option is not easy to do, and in many cases it cannot be done! With V-bands, the stored strain energy is the major shock producing part of the event. With linear shaped charges, we find the thickness of the material is probably one of the stronger parameters in determining the shock level. In some cases, efforts were made to redesign the separation system, and what happened was the separation system did not work.

Figure 4 shows the case, circa 1964-1975. It arises often in the following situation: There are two contractors; one is the payload contractor and the other is a booster contractor. Both contractors have interface criteria defined, and neither one can exceed the interface criteria because the equipment designer on one side has designed the equipment to the levels provided by the analyst on the other side. Quite often, when a combined test of both systems is conducted, it is found that the interface criteria were exceeded, and the contractor whose hardware causes the higher shock level has to do something to reduce it. The obvious solution is to insert something at the interface to reduce the transmitted shock level, which would solve the problem. In order to get significant attenuation, a flexible joint is needed; but in load carrying interfaces it is also necessary to maintain stiffness. These conflicting requirements preclude this as a practical solution. Nevertheless, as indicated on the bottom of Figure 4, on a number of programs extensive efforts were made to use Nitral rubber, felt, lead washers, fiberglass or combinations of these and other materials. Sometimes a small amount of attenuation was achieved in parts of the frequency band. Significant reduction over the total band was never achieved.

Referring to Figure 5, I am sure many of you have probably heard these statements, either case, the predicted shock levels are too high or too low. I believe these statments are symptomatic of our inability to accurately predict pyrotechnic shock levels. It is very difficult, and generally experimental data are needed to apply to each particular problem being worked. It is very easy to be off 6 dB in shock predictions. For example, recently on a program the predicted shock environment from a linear cutting type of separation system was approximately 12,000 g's. A test was run, and measured levels were on the order of 25 to 30,000 g's causing a major program impact.

Around 1969, we began to recognize that drop shock pulse test methods were not the best way to simulate the pyrotechnic shock environment because they substantially overtest at the low frequency resulting in unrealistic hardware failures. It became obvious we needed to simulate the shock more in terms of the real environment, which is an oscillatory transient. One method was to use a shaker which produces a vibrating type of environment. However, concerns existed regarding whether or not shocks could be generated using shakers and would the shakers survive. If we look at the experience part of Figure 6, we find that today shakers are routinely used to simulate pyrotechnic shocks. A level of 7,000 g's response cannot be directly accomplished on the head of a shaker. This level can be acheived if a combination of the shaker and a resonant fixture is used. Without resonant fixtures levels are normally limited to somewhere in the 2,000 to 3,500 g range.

Figure 7 shows a situation that arises when an engineer has test data from one separation test. There is a need to establish design levels that consider the test-to-test variability, or the vehicle-to-vehicle variability if you are dealing with an extended production line of vehicles. The question is, what is that test-to-test or vehicle-to-vehicle variation? The ordnance people will say that the charges are controlled to within a very few percent. Occasionally the argument is that the shock levels should not vary more than the amount that the charge in the ordnance varies. The experience is not quite that way, as can be seen at the bottom of Figure 7. I would like to emphasize one aspect that sometimes is overlooked about measured test data. That is, variability in shock data is due to test-to-test variations, but it is also due to vehicle-to-vehicle variations for the same vehicle design. A paper was presented yesterday that showed a true test-to-test variability which was much less than shown at the bottom of Figure 7. I have seen the same thing in my experience. With a particularly well controlled test set up, if the ordinance is fired a half a dozen times, the variability is small. A problem arises when testing additional vehicles of the same design. The manufacturing tolerance differences in the vehicles alone greatly expand the variabiliy. I am aware of two papers which address pyro-shock variabiliy. One was published by Terry Schoessow, I believe, at the 1974 Aerospace Testing Seminar. He concluded, that if a single set of test data were available and it was desired to estimate a 95th percentile shock level, 6 dB should be added to the nominal of the single test. Another paper presented at the Shock and Vibration Symposium about two years ago drew a similar conclusion.

Figure 8 shows "Famous Last Words" that we still hear today. This is not to say that the implication of the statement is true in every instance. These kinds of statements should be considered flags. We should be a little

skeptical of generalizations implied by such a statement. If we examine the bottom part of Figure 8, the experience indicates hammers work well for 95% of the cases. This is not to say an ordnance generated shock test is not a good test. It is the perfect test; the problem is, it is more expensive. There are personnel safety considerations, which may require a remote test site. Also a considerable amount of time and money is expended in refurbishing the test article and the ordnance.

It is interesting to look at Figure 9 because it shows, if you remember, one of the earlier figures where the "Famous Last Words" were that "our equipment will be reduced to scrap," we have come full circle. Today, if the shock level is below 1,000 g's, we are hearing people say, "our equipment has always passed it, let us ignore it, and save the Government some money." That statement can result in risk of flight failure if applied on a broad generic basis. Let me show you the flight experience. Two flight failures have occurred as a result of relatively low shock levels in the recorded data base that I have. A relay failure was caused by a 600 g shock and another problem was caused by a very low level shock of 200 g's. These programs designated H&L to maintain contractor anonymity, and are also discussed in my paper. (See reference in Figure 1.)

Now let us do some self-examination. (Figure 10) First, let us ask a general question of all of us in the Shock and Vibration Community, particularly from the point of view of the Aerospace Industry: How successful have we been? Let me define what I mean by success. Success is defined here as our collective ability to minimize or reduce flight failures. To answer that question, we should examine how well the flight vehicles have been performing. That is, what has the success/failure history been? Specifically we must answer the question at the bottom of Figure 10 to respond to the first question. I have limited my time frame to 1960 and later because that basically coincides with my experience data base. It is probably a reasonable starting point because it eliminates the higher risk period during the initial growth years of the aerospace industry.

Figure 11 is essentially out of a paper I referenced earlier. (See Figure 1.) Fourteen aerospace vehicle programs were surveyed; twelve of those were launch vehicles and two were payloads. In a given launch vehicle program there may be 30, 40, 50, launches. In the two payload programs, those were single specific payload launches. The great preponderance of the data comes from the launch vehicle area. Out of 14 programs, there were 88 failures associated with shock or vibration. To state it another way, there were 88 different flights where a failure occurred which was most likely due to either shock or vibration. Out of those 88 failures, 85 were potentially shock induced and 83 of those were on launch vehicles. The next line down in Figure 11, the 41, these are the cases where post-flight failure review teams concluded, "yes indeed there is enough evidence for the review team to conclude that the failure was shock induced." With that background, I reviewed the rest of the data, and it appeared to me there were an additional 44 failures where there was a significant probability that these failures were also shock induced. I made an arbitrary assumption. I assumed 50% of those 44 failures were shock induced. That is 22 plus the 41, with a total of 63 failures; I will refer to that number in a later figure. My reasoning for assuming 50% of those 44 failures were likely to be shock induced is that none occured during the period of high vibration environments. All occurred shortly after significant shock events when the thermal and the vibration environments were relatively benign. As to the vibration failures, there have been just three among these same 14 aerospace vehicle programs, two of those were on launch vehicles, and one on a payload.

There have been four flight failures due to relay chatter or transfer (Figure 12), and all of those resulted in catastrophic loss of the mission. That is, the complete booster and its payload were lost. Shock levels are shown, and they range from relatively low levels up to quite severe levels at 4,000 g's. All of the shock levels that you see on this and on subsequent figures, are the peak of the shock response spectrum. Generally the frequency is above 2,000 Hz. Often the frequency is not much above 2,000 Hz because the data came from flight telemetry systems which typically are limited to something on the order of 2,000 Hz.

Another class of failures, the hard failures as illustrated in Figure 13, was a real surprise to me. My first presentation of flight failures was at an IES Pyro-Shock Seminar held in Orange County, California in 1982. In my original data base, there were only two or three hard failures due to pyro-shock. As a result of the presentation at the Orange County seminar, and a similar presentation a month later at the 1982 Shock and Vibration Symposium, a number of industry people came forward and supplied additional data. Since 1982 the data base has tripled. Figure 13 shows a total of 30 failures. It is important to consider the levels. They are all fairly high levels, 3,000 g's or better at frequencies of 2,000 Hz or above. There is a theory that the failure level of hardware in shock is related to a constant velocity line. That means the shock level is numerically equal to a constant times the frequency. A good reference level above which failures are likely to occur is perhaps 0.8 times frequency. For example, at 2,000 Hz if the expected level exceeds 1,600 g's, the risk of failure becomes significant. You will notice more than 50% of these hard failures identified in Figure 13 resulted in catastrophic consequences to the mission.

The third, and last type of flight failure, as shown in Figure 14, is basically a workmanship type of problem. An example of this type of failure is when solder balls are present inside of a transistor. The shock causes the solder ball to break loose, and it floats around until it gets into an area where it can cause an internal short which fails the avionics component. There were 29 failures of this kind in seven programs. More than 50% of them resulted in catastrophic loss to the mission. The levels for this kind of problem can range anywhere from very low levels to very high levels.

To summarize, I will address the question: What were the design and the testing deficiencies that allowed these kinds of failures to occur? Figure 15 lists the major reasons. First, electronic subsystems were not tolerant of intermittents. In one case, it was a relay in a guidance system circuit. The relay chattered due to the shock, scrambled the guidance system and caused the loss of the booster. Poor separation system design was another case. That separation system had an explosive bolt, and the head of the bolt was allowed to impact against metal. The impact caused the shock, something in the order of 7,000 g's, which caused the hard failure of an avionic component. There are failures caused by components being located near the shock source. One of the earliest flight failures was caused by a relay being located near the launch release bolts. Piece part designs susceptible to internal shorts because of contamination has been a major problem area. A design change that has resulted in a large reduction of contamination kinds of failures has been passivation of the internal parts of cavity type piece parts, a transistor for example. Passivation is basically coating of internal surfaces with glass or other dielectric materials so that if the particle breaks loose, it is less likely to cause an internal short. An experimental study done about 10 years ago indicated passivation can reduce the failure rate by a factor of 20 to 1.

The testing deficiencies are shown at the bottom of Figure 15. Components were inadequately qualified. For example, in one case a relay was qualified to 100 g's, and the pyro-shock levels were in the 1,000's of g's. In many cases no system level shock tests were run, so the dynamicist didn't know what shock environments to expect. Inadequate piece-part screening is another item that addresses the piece-part problem. Industry has instituted PIND testing, Particle Impact Noise Detection. PIND testing is not nearly as effective as coating or passivation. One study indicated that a success ratio on the order of 30% to 50% can be expected with PIND testing. In other words, 30% to 50% of the parts that contained the contaminants can be identified. Last, to make up for inadequate piece part or components screening, some of the launch vehicle programs that had the large number of hard failures, have instituted component acceptance testing. Generally in these instances they are dealing with fairly high shock levels. I personally think that component shock acceptance testing is a very prudent thing to do when expected shock levels are high, for example above 0.8 times frequency.

Another "Famous Last Words" chart (Figure 16) has to do with the spacecraft part of the aerospace industry. Often we hear these "famous last words", usually by people who don't have the benefit of the booster experience, and who also don't have complete information about their own hardware. Let me illustrate by a general example. When a spacecraft is launched, it is usually unpowered. It goes through the ascent acoustic and vibration environments. Some of the separation events occur, and it is still unpowered. A few hours later, the on-orbit powering-up process is begun. If a component doesn't work, it is extremely difficult to determine what caused it. The cause-effect time relationship that exists with many of the launch vehicle failures is not available. In other words, there is not enough information to determine whether a shock failure has or has not occured. In an attempt to get better quantification of that, I reviewed a report which compiled spacecraft component on-orbit failures and selected those that were potentially shock related. In a few cases, the evaluators, or the persons who put the data into the data bank, concluded that it had been a shock failure. There were very few cases such as this, on the order of four or five. From the report I selected many additional failures as being potentially shock related mainly because they seemed to be the kind of things that booster experience would indicate, could have been due to shock. This was only done when the reviewer left the definition of the causes unknown. In summary, the failure history of spacecraft due to shock is an unknown.

We can examine the flight and vibration failures very quickly. It won't take long because there are not that many. There have only been a total of three failures as illustrated in Figure 17. In one case, a component had been flown that had not been acceptance vibration tested which violates good engineering practice in acceptance testing of aerospace vehicle equipment. In the second case, the vibration environment was predicted to be 20 g's RMS. On that program the flight vibration environment had been grossly underestimated. It was a state-of-the-art type of vehicle, and basically, analytical tools to define the environment were not available. I don't have a lot of information about the third failure. A total of three flight failures occurred in the same 14 vehicle set of data from which the shock failures were tabulated.

Figure 18 gets back to the question that was asked earlier. First, I will summarize the failures caused by shock and vibration. We can positively say there have been at least three

vibration failures. There may be others I am not aware of. One of those three failures resulted in a catastrophic mission failure. In the shock area, there have been 63 failures which is based on the discussion regarding Figure 11. Sixty eight percent of those have been mission failures and almost all of these have been with launch vehicles. The last boost vehicle shock failure occured in 1977. It indicates that the boost vehicle part of the industry has probably learned how to handle pyrotechnic shock quite well. Referring to the bottom part of Figure 18: How succeessful have we, in the Shock and Vibration community, performed? In vibration, I think we can reach over our shoulder's and pat ourselves on the back because I think we have done quite well; three failures out of many hundreds of launches. This is a very small number, and we have a clear understanding of those which did occur. In one case we violated the basic ground rules of good engineering practice. In the pyro-shock area, at least through 1977, we didn't do very well. I don't believe there is enough information to determine how well we have done in the spacecraft part of the business over any time frame.

Let's go to another question (Figure 19): Why has the failure rate for shock been so much higher than for vibration? I will discuss a few possible answers. The first is that possibly the pyro-shock environment is inherently more damaging. Maybe things just fail more due to pyro-shock. That doesn't seem to be a likely explanation. I reviewed a 1983 study (Reference 2, bottom of Figure 20) which contained a survey of failures that occurred during ground testing of avionics components on four spacecraft programs. That data indicated that when components were "qual tested", about 10% of them failed pyro-shock, and about 22% failed vibration. These data discount the idea that shock environments are inherently more damaging than vibration. The second possible reason is the lack of our ability to predict pyro-shock. This is probably a partial reason. We do have a great deal of difficulty estimating what the pyro-shock environments are. Figure 20 provides some of the strongest reasons for the higher failure rate for shock than for vibration. First, for boost vehicles, I believe we can consider these in the past tense as lessons learned have been applied, and failures have not occurred in the past several years. Regarding spacecraft, the first reason, lack of the proper design consideration for pyro-shock, can probably also be referred to in past tense because design consideration for pyro-shock has been reasonably well implemented. However, I still have problems with the lack of rigorous and consistent test requirements being applied to spacecraft programs. The 1983 study on four recent spacecraft programs (Reference (2), bottom of Figure 20), indicates that avionics components are consistently tested for vibration, i.e., 100% of the avionics components are acceptance vibration tested, and 100% are qualification vibration tested. That kind of practice is not rigorously adhered to in the pyro- shock area. Of the same four spacecraft programs, none performed acceptance shock testing, and only 58% of the components were being qualified to shock. In the boost vehicle part of the industry, I believe the percentage of components subjected to pyro-shock qualification would be closer to 100%, and acceptance somewhere between zero and 70%. In some cases boost vehicle have had bad experiences and have implemented acceptance testing of a sizable percentage of their avionics.

To close, Figure 21 provides a summary of what we can do to either maintain our level of success or to improve it? In the vibration world, it seems obvious to me we should continue doing what we have been doing as we have done quite well. Some people may argue that we are over-doing it. But I doubt if there are many people who are willing to step up and say let us relax our vibration test requirements because we are so successful. In the shock world, we should apply the lessons learned from our past experience, and they are summarized at the bottom of Figure 21. I want to refer to the last item, perform shock acceptance tests. My recommendation elicited in the paper (Referenced in Figure 3), is: When shock levels exceed .8 times frequency, then serious consideration should be given to performing shock acceptance testing of avionics equipment.

PRESUMPTIONS MADE IN THE PYRO-SHOCK WORLD

- 0 Famous Last Words (CIRCA 1959)

  "The Duration Of The Shock Environment Is Too Short To Cause Failure. A 3 Minute Vibration Test Is Much More Severe."

- 0 Experience

  The First Identified Flight Failure Induced By Pyro-Shock Occurred In 1960 On Equipment Which Had Been Qualified To High Vibration Levels. A Recent Study, (1) Identified Many Flight Failures Caused By Pyro-Shock.

(1) Moening, Charles J., The Aerospace Corporation "Pyrotechnic Shock Flight Failures" Proceedings Of March 1984 Aerospace Testing Seminar, Sponsored By The Aerospace Corporation and The Institute of Environmental Sciences.

Fig. 1 — Famous last words (circa 1959)

PRESUMPTIONS MADE IN THE PYRO-SHOCK WORLD

- <u>Famous Last Words (CIRCA 1960-1970)</u>

  "Our Electronic Equipment Will Be Reduced To Scrap If Exposed To Pyro-Shock Levels Of Several Thousand G's."

- <u>Experience</u>

  Most (Not All) Off-The-Shelf Avionics Equipment Found To Withstand Levels Of Several Thousand G's.

Fig. 2 — Pyrotechnic shock environment circa 1960-1970 time frame

PRESUMPTIONS MADE IN THE PYRO-SHOCK WORLD

0 Famous Last Words (CIRCA 1966)

"Pyro-Shock Is Caused By The Explosive Charge; Reduce The Charge And Reduce The Shock. That Will Solve Our Problem"

0 Experience

- ;ored Strain Energy Is Major Producer Of Shock In V-Bands
- Amount Of Metal Being Cut Is Most Significant Factor In Linear Cutting Devices
- The Charge Has Reduced And The Separation System Didn't Work

Fig. 3 — Problems with equipment qualified to shock levels below measured environmental levels

PRESUMPTIONS MADE IN THE PYRO-SHOCK WORLD

0 <u>Famous Last Words (CIRCA 1964-1975)</u>

"Pyro-Shock Can Be attenuated By Use Of Dissimilar Materials In Structural Interface Joints. We'll Solve The Problem That Way."

0 <u>Experience</u>

Many Development Tests With Wide Variety Of Materials Shows No Significant Attenuation Through Load Carrying Interfaces.

Fig. 4 — Problems in meeting interface pyrotechnic shock criteria

PRESUMPTIONS MADE IN THE PYRO-SHOCK WORLD

0 Famous Last Words (CIRCA 1960-1985)

"The Predicted Shock Levels Are Much Too High. (Or Too Low)"

0 Experience

Measured Data Exceeded Predictions
(Measured Data Well Below Expectations)

Fig. 5 — Problem in accurately predicting the pyrotechnic shock environment

PRESUMPTIONS MADE IN THE PYRO-SHOCK WORLD

- 0 Famous Last Words (CIRCA 1969)

  "Vibration Shakers Are Not Capable Of Simulating Pyro-Shock Of 3000 G's. We Will Have To Drop The Component From A Tall Building".

- 0 Experience

  - Shakers Routinely Used To Simulate Pyro-Shock Levels Up To 7000 g Response

Fig. 6 — Problems and experiences in simulating pyrotechnic shock

PRESUMPTIONS MADE IN THE PYRO-SHOCK WORLD

0 <u>Famous Last Words (CIRCA 1970)</u>

"The Charge In The Ordnance Is Controlled Within A Few Percent, Therefore, The Pyro-Shock Levels Will Not Vary More Than That Amount."

0 <u>Experience</u>

- Measured Data Show Test - To - Test Variation In Shock Level Of - 50%, + 100%

Fig. 7 — Experience with test-to-test variations in measured pyrotechnic shock data

PRESUMPTIONS MADE IN THE PYRO-SHOCK WORLD

- 0 Famous Last Words (CIRCA 1975-1985)

  "A Hammer Test Won't Work. An Explosive Must Be Used For Test To Simulate An Explosive Event."

- 0 Experience

  Hammers On Ringing Plates Found To Be An Acceptable Method Of Simulating Pyro-Shock

Fig. 8 — Hammer and anvil impact tests versus ordnance generated pyrotechnic shock tests

PRESUMPTIONS MADE IN THE PYRO-SHOCK WORLD

0 <u>Famous Last Words (CIRCA 1980-1985)</u>

"Avionics Equipment Doesn't Fail At Levels Below 1000g's. We're Wasting Money Testing Equipment To Such Low Levels. Let's Delete The Test Requirement."

0 <u>Experience (Flight Experience Only)</u>

Two Flight Failures Have Occurred As A Result Of Shock Levels Levels Below 1000g's

- Program H : Relay Chatter Induced by 600 g Shock Caused Catastrophic Mission Failure
- Program L : 200 g Shock Caused Degraded Mission Performance

Fig. 9 — History of flight failures where shock levels were below 1000 g's

QUESTIONS: SHOCK & VIBRATION COMMUNITY

- How Successfully Has The S & V Community Performed In The Aerospace Industry?

  Success Is Considered To Be The Ability Of Our Collective Efforts To Minimize Flight Or Service Use Failures.

- Of The Aerospace Vehicles Launched Since 1960 How Many Flight Failures Have Been Caused By Shock And Vibration Environments?

Fig. 10 — Question on the ability of the aerospace shock and vibration community to minimize flight failures

Flight Failure Data Base

- 14 PROGRAMS
  - 12 LAUNCH VEHICLE
  - 2 PAYLOAD
- 88 FAILURES
  - 85 POTENTIALLY SHOCK-INDUCED (83 on launch vehicles)
    - 41-- REVIEW TEAMS CONCLUDED FAILURES WERE SHOCK-INDUCED
    - 44-- HIGH PERCENTAGE PROBABLY ALSO SHOCK-RELATED
      - NONE OCCURRED DURING HIGH VIBRATION
      - ALL OCCURRED SHORTLY AFTER SIGNIFICANT SHOCK EVENTS, THERMAL AND VIBRATION ENVIRONMENTS BENIGN
  - 3 VIBRATION-INDUCED (2 on launch vehicles, 1 on payload)

Fig. 11 — Aerospace vehicle flight failure data base

IN-FLIGHT FAILURES TRACEABLE TO SHOCK (RELAY CHATTER/TRANSFER)

0 Relay Chatter/Transfer Caused Loss Of Boosters And Payloads

Program	Peak Shock Spectrum g's (100-10,000 Hz)	No. of Failures & Mission Effect
A	4000	1 - Catastrophic
D	Unavail.	1 - Catastrophic
G	1900	1 - Catastrophic
H	600	1 - Catastrophic
	TOTAL	4

Fig. 12 — In-flight failures traceable to shock (reply chatter/transfer)

IN-FLIGHT FAILURES TRACEABLE TO SHOCK (HARD FAILURES)

- Broken Electrical Wires, Leads, Cracked Glass Caused Failure Of Electronic Components

Program	Peak Shock Spectrum g's (100-10,000 Hz)	No. Of Failures & Mission Effect
C	3000	16 (5 Catastrophic, (11 Degraded)
F	6000	1 Degraded
I	3100	7 Catastrophic
M	3000	5 Catastrophic
N	Unavail.	1 Unavail.
	TOTAL	30

Fig. 13 — In-flight failures traceable to shock (hard failures)

IN-FLIGHT FAILURES TRACEABLE TO SHOCK (CONTAMINANTS)

0 Dislodging Of Contaminants (IC's, Transistors) Causing Electronic Equipment To Fail

Program	Peak Shock Spectrum g's (100-10,000 Hz)	No. of Failures & Mission Effect
B	3000	8 - Catastrophic
E	2800	1 - Catastrophic
I	3100	9 - Catastrophic
J	Unavailable	4 - Unavailable
K	1500	1 - Catastrophic
L	200	1 - Degraded Performance
M	3000	5 - Catastrophic
	TOTAL	29

Fig. 14 — In-flight failures traceable to shock (contaminants)

DESIGN/TESTING DEFICIENCIES LEADING TO PYRO-SHOCK FAILURES

0 Design Deficiencies

- Electronic Subsystems Not Tolerant Of Intermittents
- Poor Separation System Design (produced high shocks)
- Components Located Near Shock Source
- Piece Part Design Susceptible To Internal Shorts

0 Testing Deficiencies

- Components Inadequately Qualified
- No System Level Shock Testing
- Inadequate Piece Part Screening
- Inadequate Component Screening

Fig. 15 — Design/testing deficiencies leading to pyro-shock failures

PRESUMPTIONS MADE IN THE PYRO-SHOCK WORLD

0 <u>Famous Last Words (CIRCA 1980-1985)</u>

"We Have Never Had A Flight Failure Due To Pyro-Shock. Let's Delete The Test Requirement And Submit A Cost Saving"

By: Spacecraft Designer Whose Experience Is Based On Equipment Which Is Unpowered During Ascent

0 <u>Experience</u>

Review Of Early Orbit Failures Of 128 Spacecraft Showed 72 Failures Potentially Shock Related. The Cause Of Failure Of A Component Which Doesn't Work Upon Reaching Orbit Is Often Defined As Unknown. No Data Is Available To Establish Whether Or Not A Causal Relationship With Shock Or Vibration Exists.

Fig. 16 — Reasons for orbit failures of spacecraft

IN-FLIGHT FAILURES TRACEABLE TO VIBRATION

Program	No. Of Failures	Failure Type	Estimated Overall (grms) (20-2000 Hz)	Time Of Failure Mission Effect
B	1	Electronic Box Failure	Unknown	o Coincident With High Vibration
F	1	Electronic Box Failure	20	o Coincident With High Vibration o Catastrophic
H	1	Component	Unknown	o Coincident With High Vibration

Fig. 17 — In-flight failures traceable to vibrations

S & V Community Performance (Aerospace Industry)

Of The Aerospace Vehicles Launched Since 1960, How Many Flight Failures Have Been Caused By Shock And Vibration Environments?

- 0 Failures Due To Vibration : $\geq$ 3 (33% Mission Failures)
- 0 Failures Due To Pyrotechnic Shock : $\geq$ 63 (68% Mission Failures)
  - \- Last Boost Vehicle Shock Failure in 1977

How Successfully Has The S & V Community Performed?

- 0 Vibration : Excellent
- 0 Pyro-Shock : Not Very Well Thru 1977

Fig. 18 — Summary of aerospace vehicle flight failures caused by shock and vibration environments

QUESTION

0 Why Was (Is) The Failure Rate For Pyro-Shock So Much Higher Than For Vibration?

- Pyro-Shock Inherently More Damaging, (Not Likely)

  1983 Study (2) - 10% Of Components Tested Fail Pyro-Shock

  - 22% Of Components Tested Failed Vibration

- Inability To Predict Pyro-Shock (Partial Reason)

Fig 19 — Reasons why failure rate for pyro-shock is much higher than for vibration

CONTINUED

0 Why Was (Is) Failure Rate For Pyro-Shock So Much Higher Than For Vibration? (Strongest Reasons)

- Lack Of Design Consideration For Pyro-Shock
- Lack Of Rigorous And Consistent Test Requirements

0 1983 Study (2) Test Practices Of 4 Recent Programs

PERCENT OF AVIONICS COMPONENTS TESTED

Acceptance Vibration	100%	Acceptance Pyro-Shock	0%
Qual Vibration	100%	Qual Pyro-Shock	58%

(2) Internal Aerospace Corp. Report "Test Data Bank" By: R. B. Laube, 28 October 1983

Fig 20 — Reasons why failure rate for pyro-shock is much higher than for vibration (strongest reasons)

WHAT CAN WE DO TO MAINTAIN OR IMPROVE SUCCESS

- Continue Rigorous Specification And Test Practices For Vibration
- Apply Lessons Learned From Past Experience With Pyro-Shock
  - Design Lessons
    - Avoid Locating Components Near Shock Sources Or Protect
    - Use Low Shock Separation Devices
    - Use Piece Parts Designed To Minimize Susceptibility To Internal Shorts
  - Test Lessons
    - Qualify Components By Test
    - Perform System Level Shock Tests
    - Perform Component Shock Acceptance Tests

Fig. 21 — Recommendations for maintaining or improving level of success

PYROTECHNIC SHOCK

THE PRE-PULSE IN PYROSHOCK MEASUREMENT AND ANALYSIS

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Accelerometers are usually incapable of faithful measurement of the nearly instantaneous velocity change occurring when a structure is subjected to excitation that closely approaches a true impulse (as is often the case with pyrotechnic or X-ray induced shocks). The imperfect accelerometer behavior can lead to a serious error in the shock spectrum calculated from it. A method of correcting for one of the common accelerometer insufficiencies is provided.

NOMENCLATURE

- I = Impulse causing motion;
- M_n = Generalized mass of nth mode of structure excited;
- t = Time;
- X = Motion of measurement point;
- ζ_n = Damping, as fraction of critical damping in nth mode;
- ζ = Damping used in shock spectrum calculation;
- ϕ_{pn} = Modal deflection in direction of impulse, of nth mode at point of impulse application;
- ϕ_{qn} = Modal deflection in "X" direction, of nth mode at measurement point;
- ψ_n = Phase angle of nth modal acceleration term; $\psi_n = \cos^{-1}(1-2\zeta_n^2)$;
- Ω_n = Modal frequency.

INTRODUCTION

When a linear, viscously damped structure is subjected to an impulse I at point p, the motion at point q after the completion of the impulse application can be written as -

$$X(q,t)=\sum_{n=1}^{N} \frac{I\,\phi_{pn}\,\phi_{qn}}{M_n\,\Omega_n\sqrt{1-\zeta_n^2}} \exp(-\zeta_n\,\Omega_n\,t)\cdot \sin(\sqrt{1-\zeta_n^2}\,\Omega_n\,t) \quad (1)$$

If we were capable of measuring such motion directly and used the measurement as the input to calculate the shock or Fourier spectrum, the difficulties to be dealt with in this paper would not exist. They arise because the ordinary method of measuring shock motion is the accelerometer, and we generally use the measured acceleration to calculate the relative displacement spectrum and from it the equivalent static acceleration shock spectrum. (Ref. 1)

In the process of differentiating Eq. 1 to yield the acceleration that our instrument will be subject to, we should observe that at the instant immediately after the completion of the postulated impulse application the velocity is non-zero -

$$\dot{X}(q,o+) = \sum_{n=1}^{N} \frac{I\,\phi_{pn}\,\phi_{qn}}{M_n} \quad (2)$$

Since the velocity just before the event is zero, it should be clear that the direct result of two differentiations of Eq. 1 -

$$\ddot{X}(q,t) = -\sum_{n=1}^{N} \frac{I\,\phi_{pn}\,\phi_{qn}\,\Omega_n}{M_n\sqrt{1-\zeta_n^2}} \cdot \exp(-\zeta_n\,\Omega_n\,t)\cdot\sin(\sqrt{1-\zeta_n^2}\,\Omega_n t+\psi_n) \quad (3)$$

is not complete; it has neglected the implicit Heaviside unit function multiplying Eq. 1 and therefore conceals the very high acceleration prevailing for the very short time which is characteristic of the impulsive excitation

that was assumed at the outset and which caused the Eq. 2 velocity. The missing term of Eq. 3, (which is the "pre-pulse" of the title) is unlikely to be measured faithfully, because of both the frequency response and the ranging necessary for accurate measurement.

It is suggested that the pre-pulse is often the cause of accelerometer and/or amplifier malfunctions (zero-shift, saturation, slew-rate limiting) and when that occurs this paper can offer no procedures for repairing the defective data. When, however, the effects of the measurement system insufficiencies are limited to clipping* of the apparent magnitude of the pre-pulse, the procedures of the paper will be useful.

PROBLEM IDENTIFICATION

An equivalent static acceleration shock spectrum† whose magnitude increases linearly with frequency implies that there was a net velocity change contained in the shock event; the magnitude of the spectrum slope is the imparted velocity.

There are some shock events (most commonly, collisions or drops) where significant net velocity is imparted to the rigid body mode, but the expected net velocity change resulting from the very short duration, small net impulse of the events of primary interest in this paper is sufficiently small (and is zero if the configuration is such that there are no rigid body modes) that we should expect the shock spectrum to be dominated by the motions of the flexible modes. In most cases of pyrotechnic shock the dominance should be expected to be complete at two octaves or more below the frequency of the first structural mode. When the shock spectrum of a pyroshock continues to increase at the rate of 6db/octave through a broad frequency range it will usually be found that the velocity change implied by the sensed acceleration is implausibly high, indicating that the physical acceleration has not been measured faithfully.

In many cases, inspection of the accelerometer trace will permit an immediate identification of the cause of the excessive velocity, which might be a drift or zero-shift in the amplifier output (see, for example, many of the accelerometer traces provided in Ref. 2, with Fig. 1.A.1.8, (the third accelerogram provided in the 7-Volume document) being all too typical!) and such data should have been discarded. For the cases we propose to deal with here, however, the recorded acceleration is of the form of Eq. 3, with no obvious instrumentation system malfunction that would cause a sophisticated technician to discard the data, but there is nevertheless a significant apparent velocity change associated with the "invisible" pre-pulse. The common inability to record the pre-pulse accurately is the cause of very frequent distorted shock spectra. An approach to correcting for this is offered following.

(Given the physics of the problem outlined in the preceding sections, the reader may be led to believe we are claiming that the problem is universal, and that there are little valid data on pyroshocks available. This would be an exaggeration of my position, since the potential problem manifests itself primarily when attempts are made to make shock measurements so close to the source of the shock that structural dissipation has not mitigated the high level, high frequency content of the pre-pulse sufficiently for the remainder to be measured. A further and often equally important beneficial effect of separation from the source is that the modal velocity terms of Eq. 2, which are additive when a_p has the same sign as a_q, will be increasingly variable in sign when measurements are reasonably removed from the source because of the typically high rates of change with position of the high frequency eigenvectors of concern.)

SOLUTIONS AND RECOMMENDATIONS

When the cause of implausibly high apparent velocity change in a shock record has been identified as the inability to record the pre-pulse faithfully, the cure is obvious: one need only add to the acceleration data a short duration, high amplitude pulse with sufficient amplitude and appropriate sign so that the integral of the augmented data is zero. The added impulse should be located immediately before the nominal beginning of the data, and "zero-time" should be redefined.

(The same result will be obtained if one modifies his shock spectrum program so that, in each solution for the relative motion of the hypothetical oscillator there is provided an initial velocity equal to the negative of the velocity determined from integration.

---

* Such clipping will often be concealed by the limited frequency response of typical amplifiers which may, subsequent to the clipping that occurs in the input stage, spread the result in time while reducing its apparent magnitude.

† If the preferred form of shock spectrum should be that of the absolute acceleration, there would be a region of acceleration proportional to frequency even though there was no net velocity implied by the integral of the acceleration (or, equivalently, if the procedures of this paper had been deemed applicable and had been employed). This region of 6 db/octave would be where the spectral frequencies are less than f_0. I am indebted to David Smallwood, whose directly applicable paper, "The Shock Response Spectrum at Low Frequencies" appears elsewhere in these Proceedings, for valuable discussion on this aspect of the problem.

Depending on one's program, one or the other equivalent procedures may be more convenient.)

The results of the suggested technique are shown in the figures, which are the results of shock spectrum analysis on what is offered as a representative shock of the form of Eq. 1. The uncorrected spectrum has much higher response levels at low frequencies than the corrected one of Fig. 2; an item of equipment with critical frequencies near 200 Hz might be severely damaged if the uncorrected spectrum was assumed to be valid and was reproduced in the environmental lab, whereas the device might very well be able to withstand the "correct" spectrum easily. Conversely, if a properly measured and analyzed severe field shock had a spectral level of 50g at 200 Hz, equipment would be grossly undertested if it were subjected to the shock of the figures and it was deemed a satisfactory shock on the basis of an analysis such as that of Fig. 1 that did not account for the pre-pulse.

At high frequencies, the corrected spectrum is seen to have high spectral levels. This is a direct result of replacing the missing pre-pulse by a very short duration (12.5 usec, for the example) high amplitude pulse which dominates the high frequency spectrum. Little quantitative weight should be attached to these spectral levels, especially since they are partially a product of the time step used in the analysis, but it should be appreciated that when we concede the postulated existence of the pre-pulse we are recognizing that the correct spectrum does indeed have very high levels at the high frequencies that "feel" the instantaneous peaks. It would be necessary to establish correct values (using instrumentation capable of measuring correctly the entire acceleration including the pre-pulse) only if there were concern about the ability of equipment to withstand high frequencies, and that would be rare.

Persons using general purpose computers with software shock spectrum routines will have no difficulty in modifying their programs to implement the correction suggested above. Persons using a "black-box" shock analyzer will have to wait until manufacturers make retrofit kits available. I urge that manufacturers do indeed start to provide retrofit kits and certainly start to give their new equipment the option of correcting for the partially or completely missing pre-pulse.

When hardware or software capable of using the pre-pulse correction is available it will be necessary to use it with caution; if used indiscriminately, it may disguise but will certainly not correct for data contaminated by a zero-shift or aliasing. If used on collision data, where a significant velocity change has been indicated correctly, it will distort the results to yield a wrong spectrum. These comments are provided at the risk of belaboring the point that critical review and physical understanding will always remain necessary in any data processing and interpretation procedures; attempts to automate and otherwise remove judgment from the process are perilous!

ACKNOWLEDGEMENT

The studies leading to this paper were performed in the course of some test and analysis programs involving pyrotechnic and X-ray generated shocks, and was supported through the TRW Project Office, under Contract F-04704-84-C-0064. Thanks are due to Mr. R. Zenko, Manager of Missile Structures and Integration Department, for his cooperation and support.

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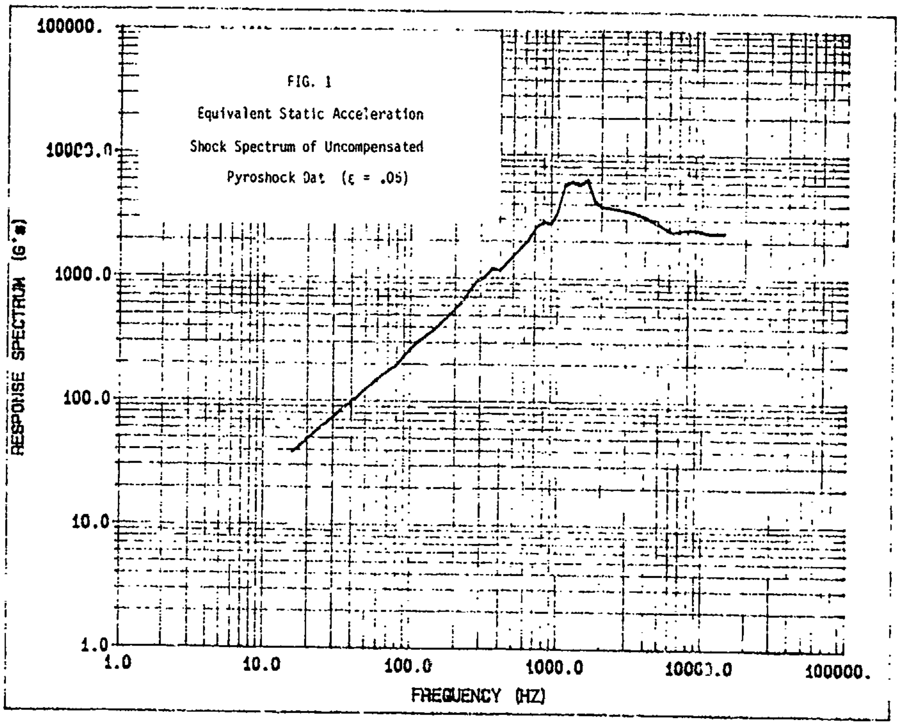

FIG. 1
Equivalent Static Acceleration
Shock Spectrum of Uncompensated
Pyroshock Dat (ξ = .05)
RESPONSE SPECTRUM (G's)
100000.
10000.0
1000.0
100.0
10.0
1.0
1.0
10.0
100.0
1000.0
10000.0
100000.
FREQUENCY (HZ)

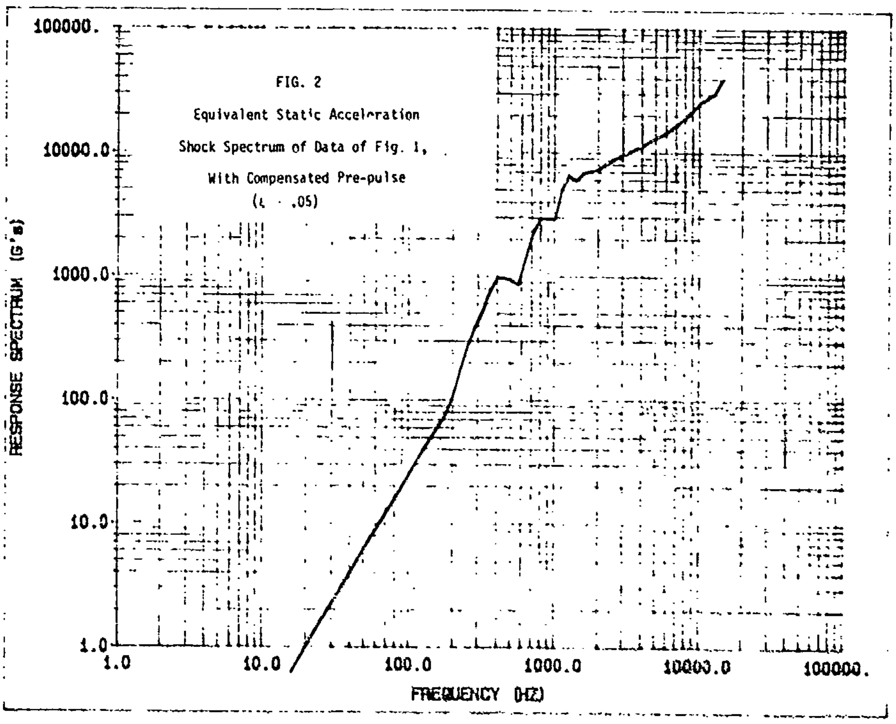

FIG. 2
Equivalent Static Acceleration
Shock Spectrum of Data of Fig. 1,
With Compensated Pre-pulse
(ξ = .05)
RESPONSE SPECTRUM (G's)
100000.
10000.0
1000.0
100.0
10.0
1.0
1.0
10.0
100.0
1000.0
10000.0
100000.
FREQUENCY (HZ)

SUPER*ZIP (LINEAR SEPARATION) SHOCK CHARACTERISTICS

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Super*Zip is a high load carrying pyrotechnic device which separates ring-shroud structure without contamination. Explosive separation devices, such as Super*Zip, employed during the spacecraft launch phase and space flight generate a shock environment that could have a deleterious effect on the spacecraft hardware. This paper presents the results of a series of tests designed to study the dynamic characteristics (and hence the shock response level) produced by detonation of the Super*Zip joint. Tests performed include separation of straight and curved panels and of complete full-ring bands for spacecraft systems. Data obtained from these tests have provided qualitative indications of the shock response levels for different test configurations. During the study, considerable effort was extended to evaluate the shock directivity, distance attenuation, boundary condition effects, and firing-to-firing variations. Representative results are shown and the information can be used as a reference base for analytical predictions as well as flight equipment design requirements.

INTRODUCTION

Super*Zip is a high load carrying pyrotechnic joint which activates without contamination. This structure cutting device is commonly used to separate missile stages and spacecraft from their boosters. It was used on the Voyager spacecraft and the Inertial Upper Stage (IUS) and is currently part of the design for the Wide Body Centaur (WBC) and the Galileo spacecraft. Such explosive separation devices employed during the spacecraft or booster launch phase and space flight generate a shock environment that could have a deleterious effect on the spacecraft or booster hardware, especially on electromechanical equipment. The environment is so complex that no analytical tool is presently available to adequately describe the basic mechanism of shock transmission and to predict shock responses. Various test programs (References 1 thru 5) have been conducted on Super*Zip devices in the past, but measured shock data is minimal and inconsistent.

In the Galileo spacecraft program, a series of Super*Zip test firings has been completed on several different configurations. These tests evaluated the capability of the Super*Zip to properly separate with margined extemes of charge grain size and temperature. During the tests, instrumentation was installed on the test articles to measure the intensity of shock due to detonation and material separation both near the joint and at other locations on the spacecraft. The shock data was analyzed to develop the pyro shock environment design and test requirements for Galileo spacecraft hardware. An effort was also expended to study the dynamic characteristics of Super*Zip generated shock, such as directivity and transmission path.

This paper describes the Galileo Super*Zip separation joint and the various test configurations, discusses the test results as well as subjects related to the shock characteristics, and offers conclusions.

SUPER*ZIP SEPARATION JOINT

Super*Zip is a full circumferential ring which joins two shroud structures. Its cross-section, as shown for Galileo in Figure 1, is a flattened tube filled with silicone rubber extrusion with a single strand HMX detonating cord molded in position. (In an earlier design for Galileo, a dual cord system was used. The change was necessary in order to reduce the possibility of tube rupture during explosion). Outside the tube, two frangible aluminum doublers with a V notch in the middle are held together by steel huckbolts as illustrated in the figure. Two detonator blocks are used to actuate the explosive charge. Table I lists typical Super*Zip dimensions and manufacturing tolerances for the Galileo application.

The explosion of the charge cord causes a bellows type expansion of the tube which cracks the doubler notch by tensile failure. In this design, the intensity of the shock generated is considerably less than other structure cutting devices such as the Flexible Linear Shaped Charge (FLSC) due to the joint type damping effects of the huckbolts holding the two doublers together. Nevertheless, the separation of the doubler creates a shock pulse which could be severe enough to cause damage and/or failure to structure or equipment located near by, and is considered to be a dominate shock generation device in the Galileo spacecraft. Determination of the Super*Zip shock environment is required in order to design and to verify the adequacy of the spacecraft system in flight operation. Experimental tests with actual firings of Super*Zip bands have been conducted to measure and to study the separation shock characteristics.

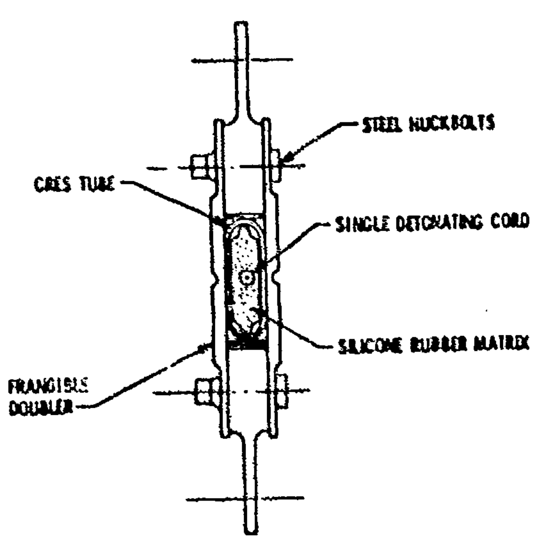

Figure 1. Galileo Super*Zip Cross-Section

DESCRIPTION OF SEPARATION TESTS

Experimental tests of Super*Zip band separation conducted in this study include separation of band segments in straight and curved panels and of complete full-ring bands for the spacecraft Development Test Model (DTM) and for the flight spacecraft systems. A complete list of the tests performed along with their results is shown in Table II.

In the panel tests, a short band segment, approximately one foot long, cut from the flight lot was used to separate the test panels. For straight (or flat) panel tests, as shown in Figure 2, two short, aluminum plates were installed along the two edges of the Super*Zip segment joint. However, during the curved panel tests, the panels, as illustrated in Figure 3, were two segments of 55 inch diameter shrouds which simulated the adjoining spacecraft adapters. The right side panel of the Super*Zip band, shown in Figure 3, is an aluminum despun ring segment and the other side is a honey-combed graphite epoxy adapter segment. The panel tests were designed primarily to evaluate the Super*Zip joint configurations being proposed for the Galileo spacecraft. In the test firings, the shock responses generated in the immediate vicinity of the detonating cord and at the outer edges of the panels were measured. While it is recognized that the dynamic characteristics (and hence the shock spectral response) of the test panels differ significantly from those of the complete ring/shell joint system, data obtained from those tests could provide qualitative indications of the relative shock levels which can be anticipated in the full-ring section.

Table I. Galileo Super*Zip Dimensions and Manufacturing Tolerance

DIMENSION OR PROPERTY		NOMINAL VALUE	TOLERANCE	
			PLUS TOLERANCE	MINUS TOLERANCE
CORD CHARGE	ℓ_c	1.1 gr/ft	0.50	-0.50
NOTCH THICKNESS	T_N	0.025 in	0.002	-0.002
DOUBLER STRENGTH	σ_D	70.0×10^3 lb/in^2	1.75×10^3	-1.75×10^3
DOUBLER THICKNESS	T_D	[illegible] in	0.004	-0.004
DOUBLER MODULUS	E_D	10.40×10^6 lb/in^2	0.05×10^6	-0.05×10^6
HUCK BOLT TO NOTCH LENGTH	L_{HB-N}	[illegible] in	[illegible]	[illegible]
MASS OF RING, BOLTS ETC.	M_{RING}	[illegible] $\times 10^{-3}$ lb-s/in^2	[illegible] $\times 10^{-3}$	[illegible] $\times 10^{-3}$
TUBE THICKNESS AND DENSITY	T_T, ρ_T	[illegible] $\times 10^{-3}$ lbm/in^2	[illegible] $\times 10^{1}$	[illegible] $\times 10^{-3}$

For the full-ring band DTM tests, only portions of the mock-up spacecraft which involved the essential parts adjacent to the Super*Zip separation band were utilized to determine the shock environments. The Development Test Model (DTM) consisted of the Galileo prototype Despun section with a mass mockup of the truss mounted electronics bay (Bay E) attached, and the Centaur upper adapter joined to the Despund section via the Super*Zip joint. All test hardware were assembled in a vertical stacked position. The test article was suspended from the support beam attached to the removable door of the test chamber, as illustrated in Figure 4. Three cushioning honeycomb pads were placed under the test article and the vacuum and temperature conditions (-38°C) in spaceflight were simulated and maintained inside the chamber during the separation band's pyrotechnic actuation. The primary objective of this test was to verify that the Super*Zip joint would properly separate when exposed to flight qualification temperatures and minimum cord grain size. Secondary objectives were to obtain measurements of the generated shock near the joint and at representative equipment locations and to compare the measurements to the results from the panel segment tests.

Finally, a full-scale flight spacecraft was assembled and tested to verify the shock environments of the previous panels and DTM tests, as well as to measure the shock response levels at other spacecraft hardware interface locations. During the test firing, the Galileo flight spacecraft, as shown in

Table II. Galileo Super*Zip Tests Summary

Test Item	Detonating Cord	Test Condition	No. of Testing Firings	Comments**/ Results
Straight Panel	10 gr/ft	-38°C	1	Low Margin/ Normal Separation
	13 gr/ft	Room Temperature	1	High Margin/ Separation but Doubler Sheading
Curved Panel	9 gr/ft*	Room Temperature	3	Normal Separation
	12 gr/ft*	Hot and Cold	2	High Margin/ Tube Rupture
	7 gr/ft	Room Temperature	1	No Separation
	8 gr/ft	-38°C	4	Low Margin/ Normal Separation
	12 gr/ft	-38°C	1	High Margin/ Normal Separation
Galileo DTM system	9 gr/ft Full 55" diameter and vacuum band	-38°C	1	Low Margin Qualification/ Normal Separation
Galileo Flight Spacecraft	11 gr/ft Full band	Room Temperature	1	Flight Operation/ Normal Separation

* Dual cord was used in earlier test program.

** Low or high margin refers to a low or high cord charge size in comparison with flight configuration, to demonstrate device separation capability.

Figure 5, was suspended by an overhead crane through the lifting eyes on the bus and was positioned one-tenth inch above the cushioning pads on the support cart. An inverted flight spare lower adapter was attached to the bottom of the spacecraft lower adapter to simulate the Centaur interface. The support trusses of all flight equipment were removed from the spacecraft adapter since they are released before the Super*Zip firing separates the lower adapter in the flight mission pyrotechnic firing sequence. A stabilizing fixture was attached to the top of the bus with two adjustable cables to support the appendage equipment. The test was successfully completed with full Super*Zip band separation and no evidence of structural damage in the Spacecraft was observed.

In all four of the above test configurations, selective sets of tri-axial accelerometers (Endevo Mode 2225A) were installed on the test articles at various locations to measure the structural shock responses. All accelerometers were stud-mounted to metal blocks and these blocks were bolt-mounted and bonded to the test hardware. The measurement locations for the test series are shown in the photographs in Figures 2 through 6. Accelerometer inputs were recorded and analyzed, and are presented as shock response spectra.

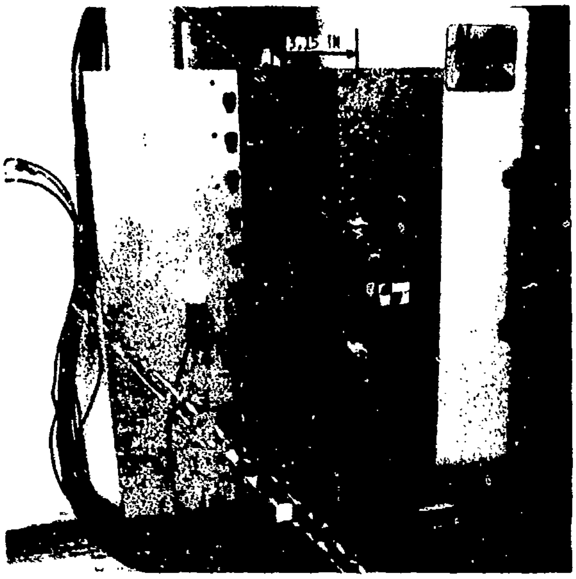

Figure 2. Super*Zip Straight Panel Test Configuration

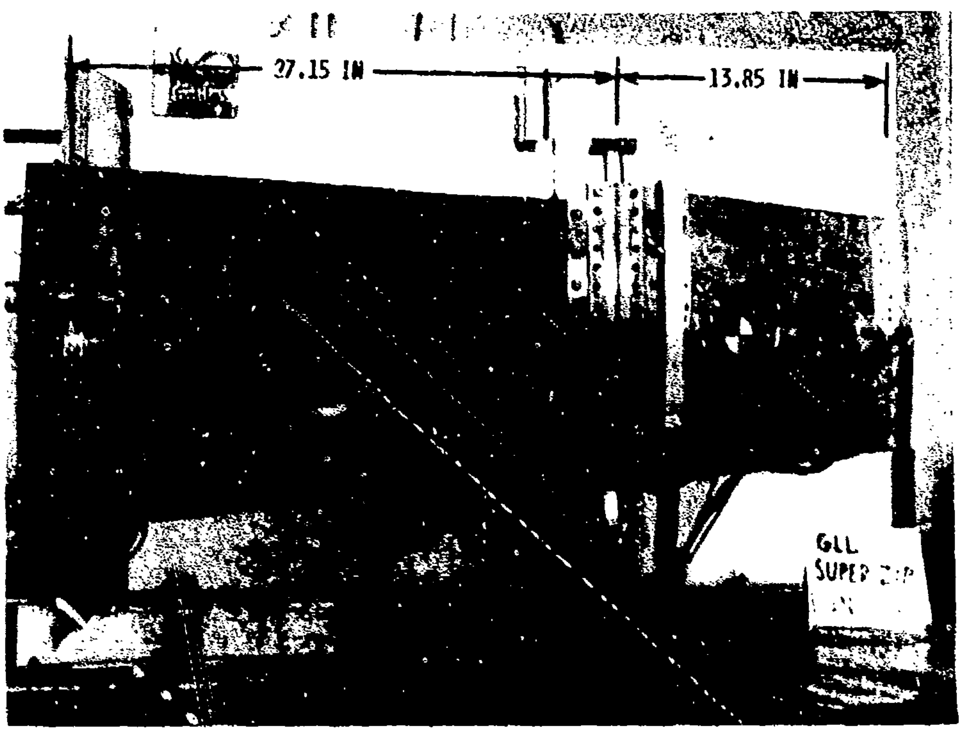

Figure 3. Super*Zip Curved Panel Test Configuration

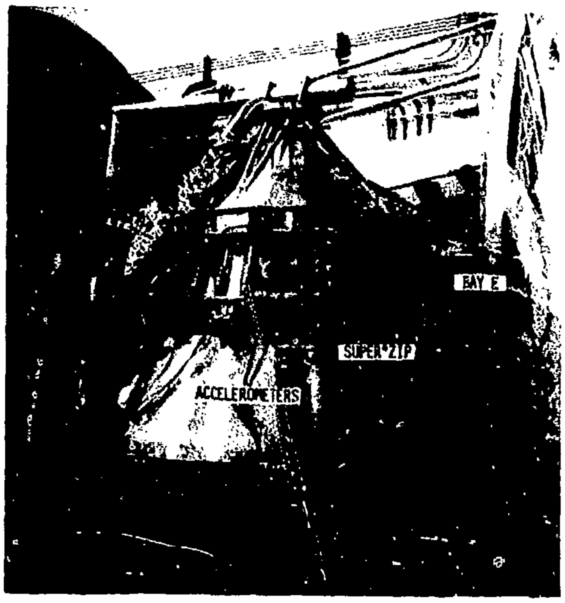

Figure 4. Galileo DTM Super*Zip Full Band Test Configuration

Figure 6a. Super*Zip Band and Adapter Accelerometer Locations

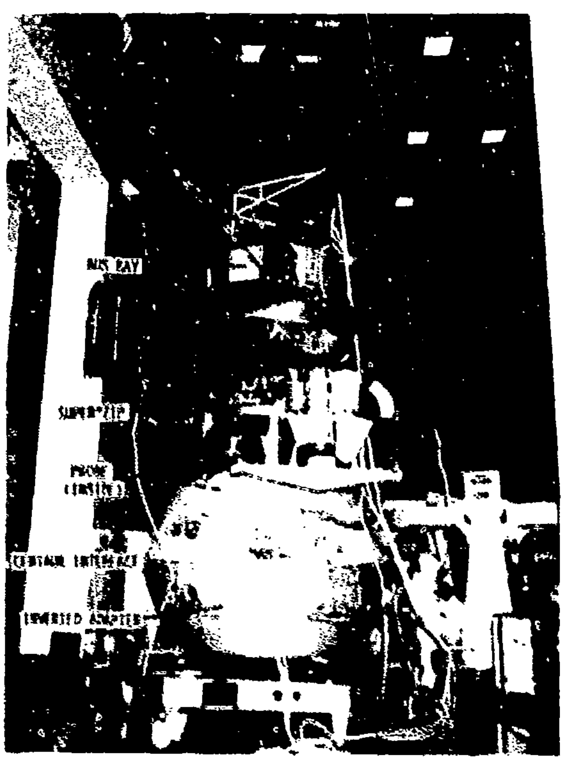

Figure 5. Galileo Spacecraft Super*Zip Test Configuration

Figure 6b. Centaur Adapter Accelerometer Locations

TEST RESULTS AND COMPARISONS

Tests were successfully accomplished and the overall test objectives were met, although some data were not satisfactorily recorded due to instrumentation problems. In the following comparisons, the shock response spectrum is used to define the shock environment for the various test configurations, conditions, and instrumentation locations. The shock spectra are analyzed from the measured acceleration responses with a dynamic amplification factor (Q) of 20.

Figure 7 compares the maximum envelopes of shock data obtained from the curved panel tests for both Super*Zip normal separation and test failure (i.e., non-separation or tube rupture). Clearly, the shock levels generated by Super*Zip detonation from normal separation are much greater than those obtained from tube rupture cases. It appears that, because of tube rupture, significant mechanical energy was absorbed by other sources.

Figure 8 illustrates the shock response levels reduced from the data measured near the Super*Zip band in four repeat panel firings with the same charged grain size (8 grain per foot single element detonating cord) and temperature (-38°C). The shock response spectrum of four firings does appear repeatable except at the low frequency portion, where one test seems to be much higher than the other three test firings. By investigating the acceleration measurement of this channel, it was found out that a "half-sine" pulse shift (or low frequency content error) existed and the data is not considered to be valid. Overall the firing-to firing variations of Super*Zip shock environments are much less than 3dB.

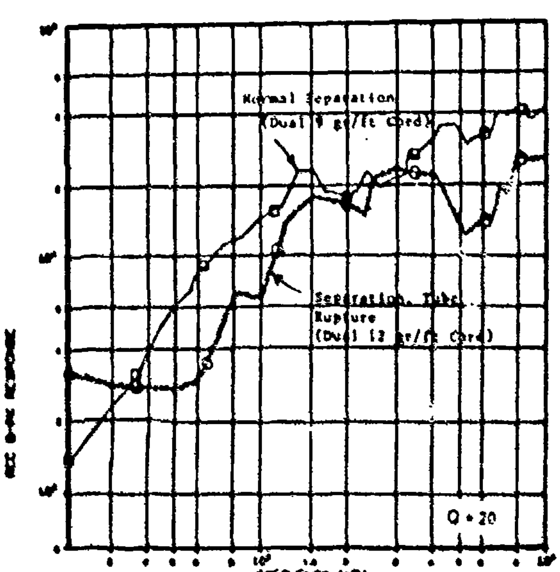

Figure 7. Comparison of Shock Levels During Super*Zip Normal Separation vs. Test Failure

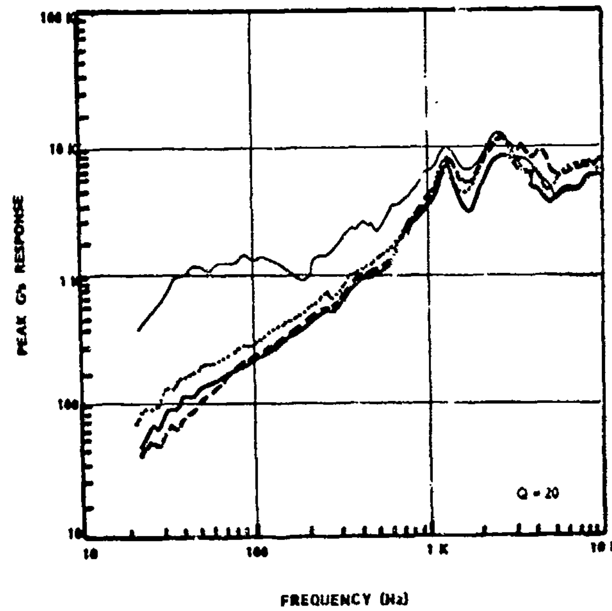

Figure 8. Shock Measurements Near Super*Zip for 8 gr/ft at -38°C

Figure 9 shows the overall comparisons of shock levels for different cord size firings. The shock spectrum levels as plotted were reduced from the data measured near the Super*Zip band (approximately within 3 inches), thus should not be affected by the variations in test configuration. It was also predicted that the effect of cold temperature (-38°C) would be equivalent to a decrease in the charged grain size by one and would have no effect on the Super*Zip function as well as the structure shock response levels. Comparisons of these data indicate that shock levels at frequencies above 600 Hz are quite similar among all test firings. However, considerable scatter in the low-frequency region is evident. Further analyses were performed and no definite relationship between shock amplitudes and charged grain size could be defined. In fact, because of the instrumentation ranging problem and prevailing signal-to-noise ratios, the data for the lower frequencies is likely to be influenced by the system noise floor. JPL test results demonstrated that during the normal separation of the Super*Zip joint, the effect on the induced shock level of increasing the charged grain size is

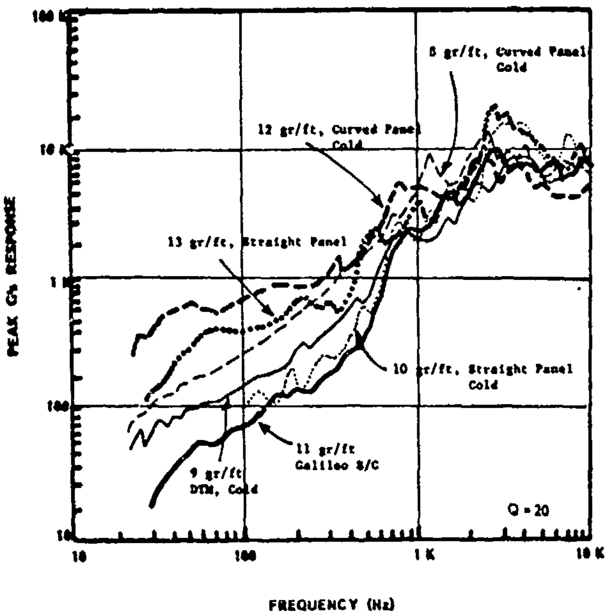

Figure 9. Shock Level Variations Near Super*Zip Band with Charged Grain Size

insignificant and can be neglected. Prior to these tests, based on analytical predictions for linear pyrotechnic devices, the higher charge was expected to increase shock levels by 1 dB for every 2 grain/ft increase (shock level is proportional to the square root of charge grain increase). Since the firing-to-firing variation of shock levels with no difference in test configuration is typically in the 1 or 2 dB range, the above tests were not able to verify this prediction.

Figures 10 and 11 include the data measured at locations further away from the Super*Zip joint. In the previous figures, the data was based on the measurements near the shock source, and as expected, no effects due to the test boundary conditions on the shock environment were noticed. As the shock pulse propagates through the structure, the response acceleration amplitude is expected to attenuate and the wave form is modified by reflections from the boundary. Near the edge of test article and at the interface of the mounting equipment, the effects of the boundary conditions could be quite pronounced. Figures 10a and 10b compare the maximum envelopes of shock spectra data obtained from both the panel and full circumferential ring tests. Clearly, the shock levels measured at both ends of the test panel during the open-panel tests are considerably higher than the full-ring tests. Both figures show a similar higher shock level (above 9 dB or higher) at frequencies above 1 KHz. This is probably the result of shock waves reflected back from the two open edges which run perpendicular to the Super*Zip joint. However, the test results also indicate that the additional structure attached on the edge of the shell adapter for the spacecraft test has relatively little effect on the shock environment. This is demonstrated in Figure 10a for the measurement at the forward ring frame location. (Data at the upper adapter frame from the full-scale Spacecraft test was not available for comparison due to instrumentation failure). This result was contrary to expectations. It was anticipated that the shock environment at the shroud edge would be affected by weight differences. No explanation can be concluded.

Also, it was predicted that the shock levels would be higher in the direction perpendicular to the test panel or in the radial direction of the shell structure. In Figure 10b, shock response levels in three perpendicular directions, at a distance away from the Super*Zip joint, are shown. As can be seen from the figure, all three responses are quite pronounced. However, by reviewing the test data, some variation in the shock response spectra for the different measurement locations could be observed. Further comparisons were performed to determine whether or not a trend existed which defined the shock propagation direction. No specific relationship could be defined. It was only found that the shock levels in the longitudinal direction (perpendicular to the Super*Zip joint) are the strongest in the high-frequency range (above 3000 Hz) and the shock in the radial direction dominates the middle-frequency range (between 1K Hz to 3K Hz). Thus, there is an indication that the tangential accelerations are slightly smaller than either the radial or longitudinal accelerations.

Figure 11a shows the overall comparison of the maximum envelope of shock response levels at several different measurement locations in the shell structure during the Super*Zip detonation. Normally, one would expect that the shock level will be attenuated from the source as measured along the shock propagation path. However, the results show that the levels are virtually constant over the entire shell adapter (i.e., compare the Despun Section and the Upper adapter), and only a slight reduction observed at the Centaur Interface which is a far distance below the Super*Zip separation plane (approximately 100 inches from the Super*Zip joint). This is probably due to the fact that the Super*Zip pyro device is a line source instead of a point source. A point source propagating into a plane decays at the rate proportional to the travel distance, while a line source propagating into a plane decays considerably more slowly, or virtually

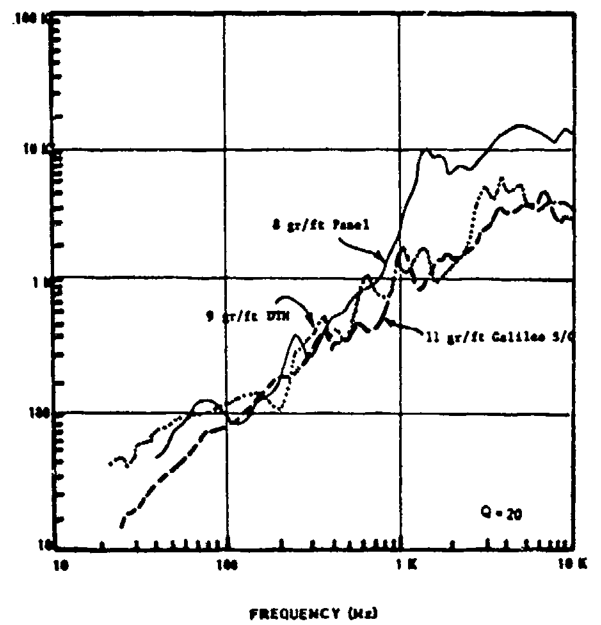

Figure 10a. Shock Environment at Forward Ring Frame (Reference 6)

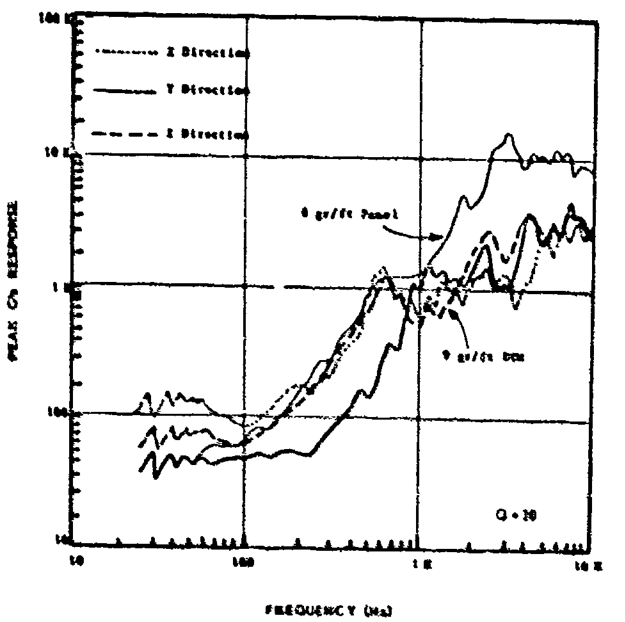

Figure 10b. Shock Environment at Upper Adapter Frame (Reference 6)

no decay over short distances. The only significant attenuation was noted between the shock levels in the immediate vicinity of the Super*Zip and the remainder of the shell adapter. This reduction (approximately 6dB) has to do with the joints the shock must travel through to reach the adapter.

Figure 11b shows the maximum envelopes of the shock response measurements at the interfaces of strut support locations (Probe and Bay E, illustrated in Figures 4 and 5). The support struts are shock isolation structures. As the shock wave propagates through the strut and reaches the equipment package, the acceleration amplitude is reduced by a considerable amount. Comparison of this figure with the response levels shown in Figure 11a shows the shock levels in the strut are about 8 dB less than the responses measured at the shell adapter. Figure 11b also shows a response measurement at the Bus location. This shock response spectrum illustrates a typical structural response in the flight spacecraft during the Super*Zip band separation. The Bus is located some distance above the shock source. The shock responses are affected by the local structure resonance as well as the structural interface joints. The shape of the shock spectrum is highly dependent on the structural transmission path and is primarly dominated by the local dynamics characteristics. For example, the high shock response of the Bus at the frequency around 800 Hz is probably the local resonant frequency of the structure.

SUMMARY, CONCLUSIONS AND RECOMMENDATIONS

The experimental test program designed to study the Super*Zip shock characteristics went as planned and the data obtained from these tests have provided qualitative indications of the shock environments for different configurations. This study reported herein indicates the following conclusions and recommendations.

- Firing-to-firing variations of the shock response levels during the Super*Zip band separation are quite small and are typically less than 3 dB from all test firings.

- A higher grain cord was selected for the Galileo spacecraft for assurance of full separation charge. The increase from 9 to 11 grain per foot was expected to increase shock levels by about 1 dB. This increase is not observed in the test results. In comparison with the firing-to-firing variations, the effects on shock levels of increasing cord sizes can be negligible.

- Changing test temperature conditions were expected to produce changes in the

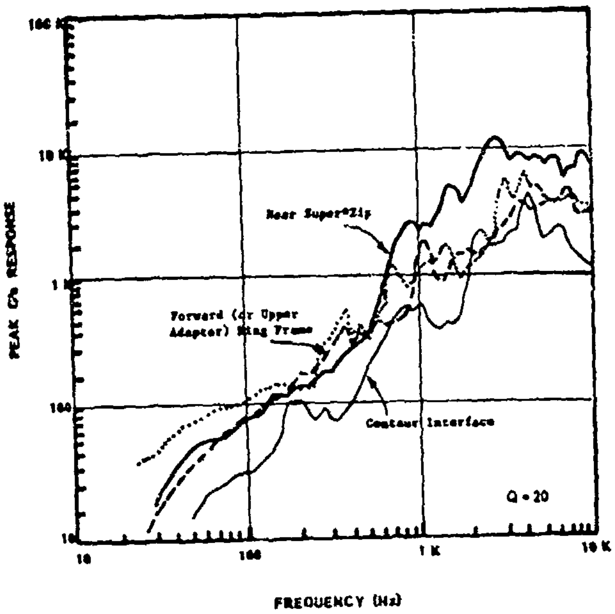

Figure 11a. Overall Comparison of Super*Zip Shock Response Levels

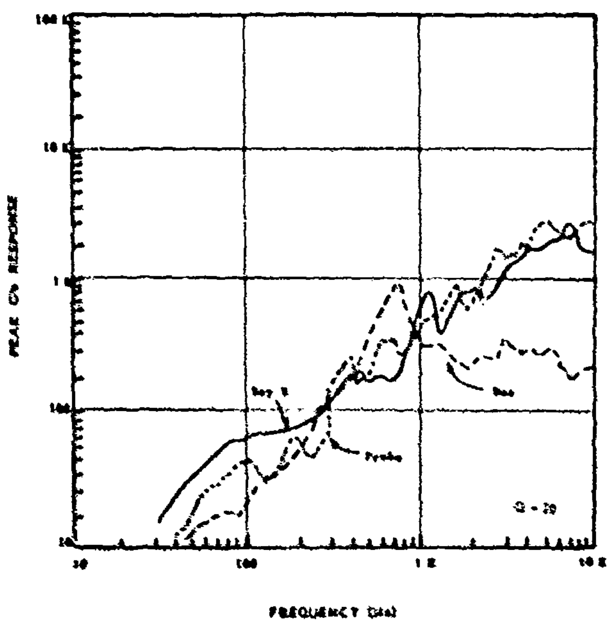

Figure 11b. Shock Environments at Various Equipment Locations (Reference 7)

shock environment, but test results show that the effects on shock responses due to temperature are insignificant. (The change of temperature on the Galileo Spacecraft operation is between -38°C to 152°F.)

- Effects of boundary conditions on shock response at a distance away from the source (i.e., Super*Zip joint) are quite significant for the open-panel configuration. No significant difference was observed in shock measurements between the partial stack-up and full Spacecraft tests.

- Distance attenuation of the shock pulse, generated by Super*Zip detonation and propagating into the shell-type structure, is extremely small and can be disregarded in short shell adapters.

- Shock responses in all three directions that were measured are quite pronounced. There is an indication that the tangential acceleration, at a distance away from the shock source, is slightly smaller than the other two directions.

Figure 12 is a summary of the shock environment that was used for Galileo spacecraft equipment design. Basically, three locations were sampled: 1) the immediate vicinity of the Super*Zip joint, 2) any place else on the shell adapter, and 3) equipment attached to the shell adapter through standard types of struts. These envelopes of the measured shock spectrum curves as presented along with the other dynamic characteristics described herein can be used as guidelines applicable to flight equipment design requirements in relation to the Super*Zip separation shock problem and is typically how JPL approaches the problem.

Acknowledgements

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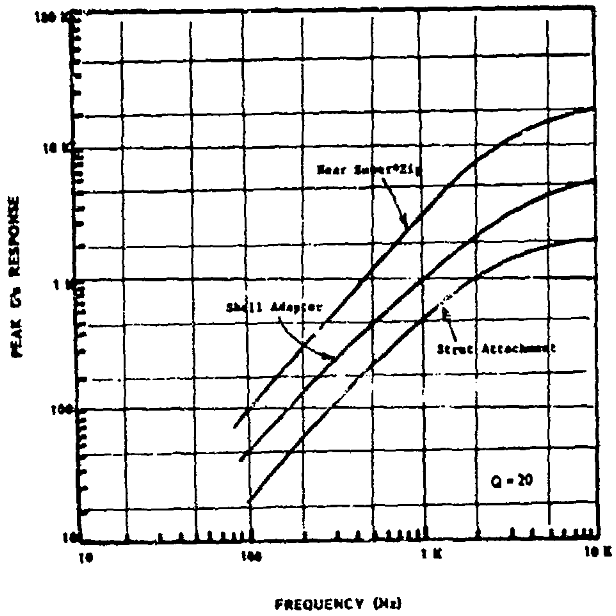

Figure 12. Shock Environment for Equipment Design

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Discussion

Voice: I noticed the higher charge gives less acceleration below one kilohertz. Thirteen grams per foot gives less acceleration than eight grams per foot. Is there a rough explanation why? There is more energy in 13 grams per foot than eight grams per foot.

Mr. Chang: I know what you are talking about. I guess that is typical. There is probably noise in the low frequency range, so we are not sure of the level.

NUMERICAL SIMULATION OF ATLAS-CENTAUR STAGE-SEPARATION SHAPED CHARGE FIRING AND STRUCTURAL RESPONSE

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Numerical simulations have been made of a flexible linear shaped charge missile stage separation system. Both the shaped charge firing and the missile structural response are modeled in detail. The numerical approach is verified by good agreement with laboratory ballistic pendulum tests and full scale stage separation tests. Parameter studies with the model have helped to identify a particular sensitivity of peak stress to a gap dimension.

INTRODUCTION

In June 1984 an Atlas-Centaur launch vehicle, flight AC62, experienced an in-flight failure due to a leak in the liquid oxygen propellant tank immediately after the firing of the stage-separation shaped charge. This paper describes some numerical simulations of the stage separation event which were made in support of a failure investigation, and compares the results of the calculations with full scale tests conducted at the General Dynamics Sycamore test site. The simulations characterize the dynamic stresses in the vehicle after the shaped charge firing and show the dependence of these stresses upon the parameters which may have a range of values or which were different in flight AC62 than in other flights.

Figure 1 shows a simplified cross-sectional view of the Atlas-Centaur stage-separation system. A flexible linear shaped charge encircles an aluminum forward adapter ring (also called the interstage adapter ring, or ISA ring) and is aimed radially inward toward a blast shield, which protects the under-lying liquid oxygen tank. When detonated, the shaped charge cuts the adapter ring. The probable failure point was determined to be near station 415. (The station number is an axial coordinate, measured in inches, in a frame oriented from the forward to the aft end of the vehicle.)

The dashed lines in Figure 1 indicate an alternate design, in which the aft part of the ISA ring has been undercut. This modification was made partly as a result of the present study and was used in the subsequent flight, AC63. The reason for the modification is that it reduces the speed with which the aft part of the cut ISA ring impacts the blast shield, which in turn reduces the dynamic stresses in the tank.

Numerical Method

The calculations were made with the PISCES 2DELK computer program, a two-dimensional general purpose finite difference program for problems involving transient stress waves (Reference 1). PISCES has been used extensively to predict the performance of shaped charges (Reference 2) and other ordnance devices (Reference 3), and in the nuclear industry it has been used to predict fluid-structure interactions (Reference 4). It is therefore well suited to simulating the transient stresses in the Centaur tank due to shaped charge firing.

In PISCES calculations, an analyst may choose to represent material with a thin shell, a continuum Lagrangian, or Eulerian formulation. The Lagrangian formulation uses quadrilateral continuum

elements to follow the motion of material undergoing moderate deformations, and can follow severe deformations with the aid of rezoning techniques. The Eulerian formulation uses a mesh which is fixed in space, and is appropriate for fluid flows. Eulerian and Lagrangian meshes can be rezoned, and Lagrange meshes can be mapped into Euler meshes when required. Both of these rezoning features were used in the course of the stage-separation calculation to improve accuracy and reduce cost.

The very large variation in time scales for the stage-separation problem required the use of two separate, coupled numerical models. An "early-time" model covered the details of the shaped charge detonation and initial tank loading out to a time of about 100 microseconds (μs). A "late-time" model was used to continue the solution out to 10 milliseconds. The late-time model included the entire liquid oxygen tank so that long time fluid-structure interaction effects would be included in the simulation.

EARLY-TIME CALCULATIONS

The perspective drawing in Figure 2 of the region in the vicinity of the linear shaped charge shows staggered spot welds, rivets, bolted components, and stiffeners that are non-axisymmetric. In addition, there are numerous other asymmetries not shown in the figure, including circumferential gaps in the ISA ring as well as variations of the tank thickness in the circumferential direction due to doubler plates at seams between gore sections. Despite these asymmetries, axial symmetry was assumed for both the early- and late-time calculations. Although we could not hope to reproduce the local stresses near asymmetry points with this approximation, we expected the axisymmetric model to give a good indication of the dynamic tank stresses away from asymmetry points because the stage-separation is essentially an axisymmetric event. The comparison with the Sycamore test data in the next section confirms this expectation.

The welded, bolted, and riveted components were idealized as being rigidly joined together in the model. Since this idealization neglects the damping and dispersion of waves which is expected to occur as small gaps open and close, the consequence of this assumption will be that the computed peak stresses may be somewhat overestimated by the model, particularly the higher frequency peaks.

Figure 3 is a global view of the computational meshes representing the region in the vicinity of the shaped charge. The boundaries of the mesh were far enough from the region of interest that no artificial boundary reflections could return in the duration of the early-time calculation.

To simplify the early-time analysis, the initial static stress in the tank was taken to be zero. (The appropriate initial static stress state was included in the late-time model, however.) The consequence of this simplification is considered to be negligible.

The calculations all began with Lagrange meshes in the vicinity of the shaped charge. After 1 μs the liner and explosive were transformed from Lagrange to Euler zoning. This was done to accurately follow the deformation of the jet and the gas-dynamic flow of the detonation products during the penetration of the ISA ring.

The initial Euler mesh, containing both the explosive and the liner, was maintained until 8 μs after detonation. At that time the zone dimensions were increased by a factor of two, and the mesh boundaries were extended to cover the entire cavity between the shaped charge confinement and the ISA ring. The change decreased the computational cost significantly without affecting the computed trend in the pressure history of the explosive. The Euler cell dimensions were doubled again at 20 μs. For calculations that ran longer than 40 μs, the Euler mesh was dropped along with the confinement, since virtually all of the impulse had been delivered to the structure by that time.

Table 1 lists the sequence of loading events in a calculation which is representative of AC62 conditions. This calculation, which is referred to as the "ISA Impact" calculation, had an ISA ring/blast shield gap of 0.127 mm (0.005 inches).

About 0.5 μs after detonation, a shock was transmitted through the fiberglass body and into the ISA ring. The shaped charge jet and the edges of the liner impacted the ISA ring at about 1 μs. The jet penetrated and separated the ISA ring in the interval 1 μs to 2 μs. The slug wedged

in the slot created by the jet, blocked the venting of the explosive through the slot, and pushed against the walls of the slot for a few microseconds. As the slot opened, explosive gases, followed by the slug, emerged from the rear of the ISA ring to impact on the blast shield. Meanwhile the very intense initial impact stresses had propagated indirectly through the aft tank ring (identified in Figure 2) to accompany the direct stresses.

The liquid oxygen contained in the tank propagated pressure waves which interacted with the stresses that move along the skin. The explosive pressure continued to load the ISA ring and to launch the confinement mass during the last part of the calculation.

Table 1. Load paths in the ISA impact shaped charge calculation. Influences on peak stresses were deduced from the late-time calculations. The amounts of momentum transferred along selected paths are indicated.

Loading Event	Interval	Influence
Transmission of detonation shock through confinement to ISA ring	0.0 to 0.5 μs	Small
Impact of edges of liner onto ISA ring	1.1 μs	Small
Jet impact and penetration through ISA ring (80 kg-m/s radial, 20 kg-m/s axial)	1.1 to 2.0 μs	Accelerates ISA ring
Motion of the aft tank ring	2 to 20 μs	Secondary
Impact of slug and early expansion of explosive (20 kg-m/s radial, 30 kg-m/s axial)	4 to 8 μs	Accelerates ISA ring, increases tank tension
Impact of residual jet and explosive gases onto blast shield	4 to 12 μs	Small
Impact of aft part of ISA ring onto blast shield (40 kg-m/s radial)	8 μs	Causes high peak stress
Impact of forward part of ISA ring onto blast shield (20 kg-m/s radial)	18.5 μs	Secondary
Compression of LOX	8 to 30 μs	Secondary
Late expansion of explosive (20 kg-m/s radial, 80 kg-m/s axial)	8 to 40 μs	Increases tank tension
Contact between tank skin and LOX (120 kg-m/s radial, 30 kg-m/s axial)	8 to 40 μs	Secondary

Results of Parameter Studies with the Early-Time Model

Parameter studies were made with the early-time model to study the sensitivity of the results to such variables as the ISA ring/blast shield separation distance, the type of blast shield support, the distance between the blast shield and the tank, and the tank contents. The key conclusion regarding the effect of the ISA ring/blast shield separation on the peak stresses will be illustrated by comparing the "ISA impact" calculation, mentioned above, with a calculation of the undercut ring configuration, which had an ISA ring/blast shield gap of 1.52 mm (0.060 inches).

Figure 4 is a mesh plot of the region in the vicinity of the shaped charge with symbols marking some points of interest. The radial velocities

of the points labeled with squares are compared in Figure 5 for the ISA Impact and Undercut Ring calculations. The velocity of the aft part of the ISA ring was at its peak at the moment of impact for the ISA Impact calculation. The impact velocity was much lower for the Undercut Ring calculation (bottom plot).

In the ISA Impact calculation, the aft part of the ISA ring impacts the blast shield at about 8 μs. It has a relatively high velocity at the moment of impact which is transmitted to the blast shield and the tank skin. This contrasts with the undercut ring configuration, where the aft part of the ISA ring impacts the blast shield much later, at 83 μs, and the velocity at impact is much lower.

The peak velocity of the tank skin beneath the blast shield is plotted in Figure 6 for the two calculations. The peak velocity is much lower for the undercut ring, and the continuation of these calculations with the late-time model showed that the peak tank stresses are also lower for the undercut ring.

Parameter studies of tank contents were made because the Sycamore tests used liquid nitrogen instead of liquid oxygen in the tank. The presence of liquid nitrogen in place of liquid oxygen increased the peak velocity from 20.7 to 23.8 m/s (68 to 78 fps) due to the lower impedance of liquid nitrogen, so the Sycamore tests should have overestimated the peak stresses and strains. A calculation was also made with an empty tank to simulate a large bubble of helium gas situated directly under the tank skin, and it produced the highest peak tank speeds. (The actual occurrence of such a bubble was considered to be unlikely, however.)

Verification of Shaped Charge Performance Calculations

The computed performance of the shaped charge was verified with tests against an ISA ring mockup and with ballistic pendulum experiments. The tests were made with a given lot of shaped charges that was considered typical of the ones used in flight AC62.

The mockup included sections of the ISA ring and the two layers of the two-piece blast shield attachment (see Figure 1). The blast shield attachment was pitted by residual jet and its surface was spattered with material. The left (front) side of the ISA ring was bent by the action of the shaped charge and its profile is in good agreement with the computed shape at 20 μs (Figure 7).

In the pendulum test, illustrated in Figure 8, a short length of the linear shaped charge was fired into a suspended metal block. The momentum delivered to the block was derived from a measurement of the height that the pendulum swings.

The observed momentum was 241 kg-m/sec when scaled to a linear shaped charge length of 962 cm, the circumference of the shaped charge when employed against the ISA ring. The calculated momentum for the ballistic pendulum simulation was 247 kg-m/sec, and the good agreement validated the shaped charge model used in the calculations.

LATE-TIME MODEL

Figure 9 shows the model used in the late-time calculations. The liquid oxygen, helium, and insulation were computed with a Lagrange mesh, and the tank parts were computed with thin shell grids. Slip was allowed between the liquid oxygen and the tank. The helium was assumed to be entirely at the top of the tank. Fluid-structure interaction was neglected in the hydrogen due to its relatively large distance from the failure location.

The numerical model necessarily neglects weld details. In regions of the structure where shells are overlapped, sliding and gap opening were not allowed, and the bending stresses were computed by each shell independently, rather than based upon the full thickness of the layers. It would have been more conservative to use the full thickness for bending in regions where welds are closely spaced axially, but the results at station 415 (the likely failure location) are probably affected very little by the choice of shell bending thickness, since station 415 is in a single thickness region.

Prior to shaped charge firing, the Centaur tank is stressed by the internal pressures in the liquid oxygen and hydrogen tanks and by the thermal stresses induced by the low temperatures. This initial stress state was found in the late-time model with the method of dynamic relaxation. The late-time model was started by driving each of the nodes near the shaped charge with the velocities computed with the early-time model. The nodes were then allowed

to move without constraint after the time of completion of the early-time calculation.

Comparisons with Sycamore Tests

Two full-scale Sycamore tests were compared with numerical results. A test which took place on 1 March 1985 had a 0.28 MPa (41 psi) tank pressure and used an ISA ring which was undercut from between 0.76 to 1.52 mm (.030 to .060 inches), and it will be referred to here as the "AC63 conditions" test. A test which took place on 13 March 1985 had a 0.35 MPa (51 psi) tank pressure and used the original ISA ring, which had a nominal gap size of 0.127 mm (0.005 inches) between the aft edge of the ISA ring and the blast shield. This test will be referred to here as the "AC62 conditions" test. The major differences between the Sycamore test conditions and flight conditions are that the Sycamore tests used liquid nitrogen rather than liquid oxygen and were at 1-g rather than zero-g. Measurements from the two Sycamore tests were compared to available calculations which most closely matched their conditions. The first of these two calculations is a true representation of the Sycamore test conditions, with a tank of liquid nitrogen, gravity, and a model of the support structure. The second is representative of flight conditions rather than test conditions, since it was made with a tank of liquid oxygen and zero gravity. However, these differences are thought to be of secondary importance as far as the initial response at station 415 is concerned, so a meaningful comparison could be made.

The Sycamore tests were instrumented with both high frequency (20000 Hertz) and low frequency (4000 Hertz) strain gages. The numerical calculations were carried out with a time step of 0.9 μs and therefore contain frequencies considerably above 20000 Hertz. Since the computed stresses near station 415 did contain a significant high frequency component, they were filtered in order to make a meaningful comparison with the test results. Figure 10 shows the effect of filtering on the response at station 415 for AC63 conditions test. The peak stress is reduced from a peak of 1.16 GPa (168 ksi) in the calculation to 0.897 GPa (130 ksi) with the 20000 Hertz filter and to 0.724 GPa (105 ksi) with the 4000 Hertz filter.

Figure 11 compares the computed and measured meridional stress for the AC63 conditions test at gage 3, which is located at station 415 in the single thickness tank skin region between gore doublers (see Figures 1 and 2). Strain gage measurements were made on both the inside and outside tank surfaces. The agreement of the inner and outer gages is remarkably good, with the main feature of a bending wave arriving at a time of 300 μs clearly seen in both calculations and test. The peak computed stress at the outside surface was 0.897 GPa (130 ksi) as compared to the measured peak of 0.827 GPa (120 ksi), and the minimum computed stress on the inside surface was 0.31 GPa (45 ksi) as compared to a measured 0.124 GPa (18 ksi).

Figure 12 compares the computed and measured meridional stress for the AC62 conditions test at gage 3 (station 415). As in the AC63 conditions test, the agreement of the inner and outer gages is remarkably good, with the main feature of a bending wave arriving at a time of 300 μs clearly seen in both calculation and test. The computed 0.97 GPa (141 ksi) peak stress at the outer surface compares well with the 0.99 GPa (144 ksi) level seen in the test, and the computed minimum stress of 0.17 GPa (25 ksi) on the inside surface is in good agreement with the 0.23 GPa (34 ksi) measured value.

To summarize, the agreement between the calculations and the Sycamore tests is quite good, especially considering the axisymmetric assumption and the shell junction simplifications that were used as well as the uncertainty in the ISA ring/blast shield gap dimension.

Peak Stresses

All calculations showed a peak stress occurring at station 415, or very close to it. Figure 13 shows the peak hoop and meridional stresses in the calculation of AC62 conditions. These peak stresses are the maximum surface stresses seen at each location during the 10 millisecond duration of the calculation. There are several peaks in the meridional stress. The highest peak occurs at station 415. Another peak occurs near station 410 in the structural cylinder which carries the load between the two tanks. Near the aft end of the tank, there is another peak which is the result of a simple reflection of the initial membrane stress wave at the massive motor ring located there. The width of this peak is approximately equal to one half of the width of the initial stress pulse, and its height above the static

curve is about twice the amplitude of the initial wave. The hoop stress peak near station 415 is quite low compared to the meridional stress in this and all other calculations that have been made. This was also observed in the tests.

Results of Parameter Studies with the Late-Time Model

Calculations made by driving the late-time model with different loading conditions showed that the peak stress at station 415 increased approximately linearly with the peak inward radial velocity imparted to the tank due to shaped charge firing. Since the early-time model showed that the radial velocity imparted to the tank depends critically upon the size of the gap between the ISA ring and the blast shield, the size of this gap is a critical factor in determining the peak tank stress, and this is one of the major results of this study.

Since flight AC62 used a higher tank pressure than previous flights, parameter studies with different initial tank pressures were made. They showed that peak stresses generally differed by less than the difference in static stress levels, so the tank pressure is not a critical factor in determining peak stress.

SUMMARY

We constructed an analytical model of the Centaur tank and flexible linear shaped charge, and used the model to simulate the dynamic stresses in the tank during stage-separation. The model contains several simplifying assumptions, the main one being two-dimensional axial symmetry, and another being the neglect of sliding and gap opening between structural components.

The early-time model was checked by applying it to a ballistic pendulum test, and very good agreement was achieved. In addition, the early-time model produced ISA ring deformations similar to those seen in experiments.

The late-time model, which was driven by the early-time model, was in good agreement with the Sycamore tests, giving credibility to parameter studies made with it.

The parameter studies with the model suggested that the peak tank stresses are most sensitive to the size of the gap between the aft part of the ISA ring and the blast shield. Calculations made for a modified ISA ring verified that it produces lower peak stresses than the unmodified ring.

All calculations showed that a moderately high stress, in the range 0.86-1.24 GPa (125-180 ksi), occurs in the skin of the liquid oxygen tank within 300 μs after firing. This stress is not considered to be high enough to have caused the tank leak experienced by flight AC62, however, since the tank material has a yield point of about 210 ksi and an ultimate strength of about 300 ksi.

The most probable cause of the tank leak is thought to be an augmentation of the detonation by external solid oxygen, a discussion of which falls outside the scope of this paper. These calculations are relevant to the detonation augmentation hypothesis, however, since they have shown that a dynamic stress peak occurs at station 415. An augmentation of the detonation would be expected to raise this stress peak without changing its location, so the leak experienced at station 415 is consistent with our results.

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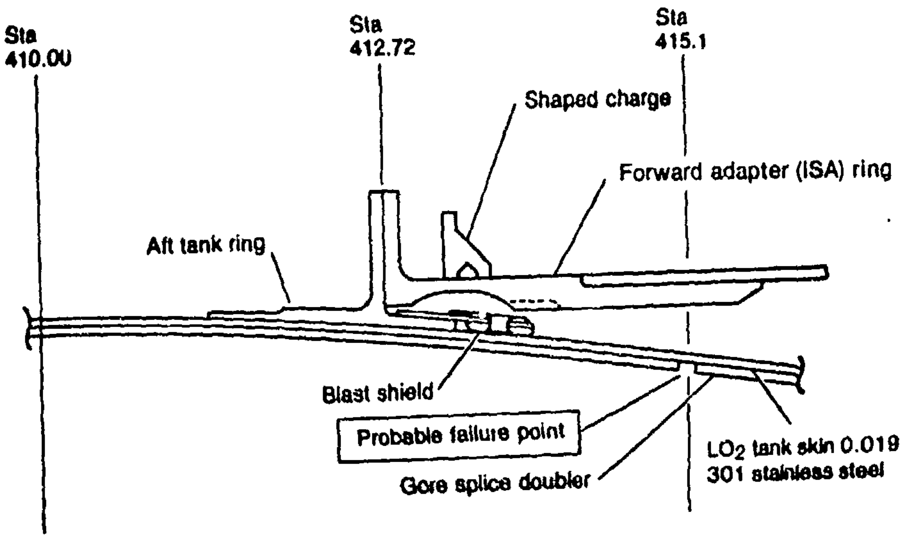


Figure 1 Shaped charge stage separation system.

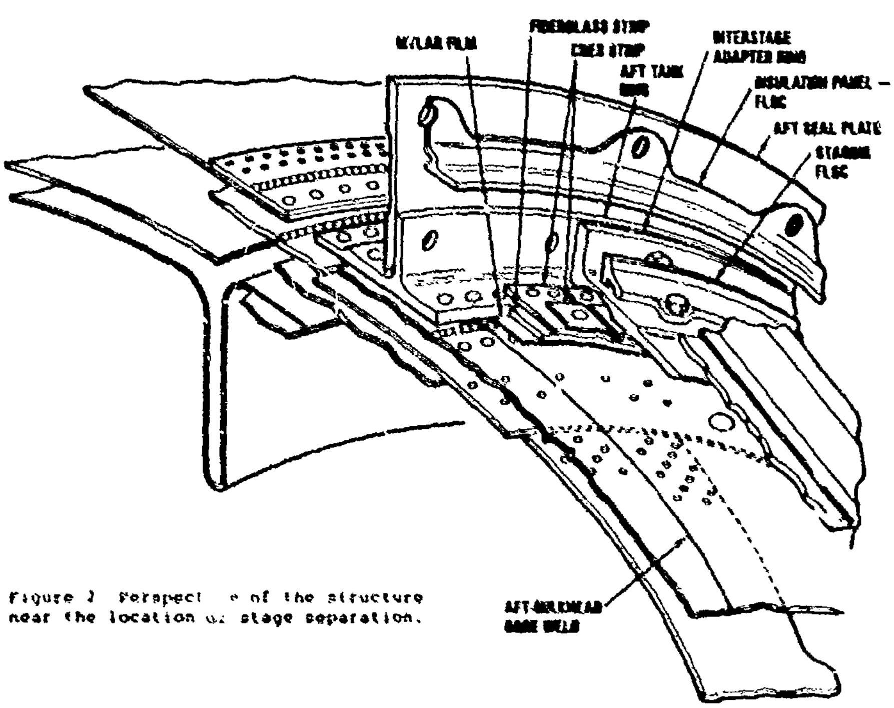


Figure 2 Perspective of the structure near the location of stage separation.

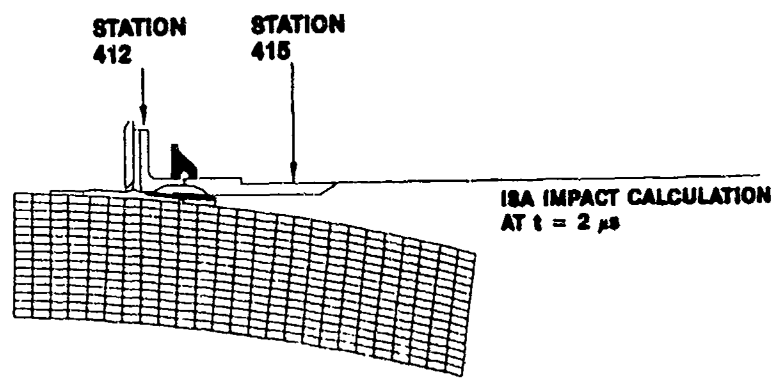


Figure 3 Global view of the computational meshes in the calculations.

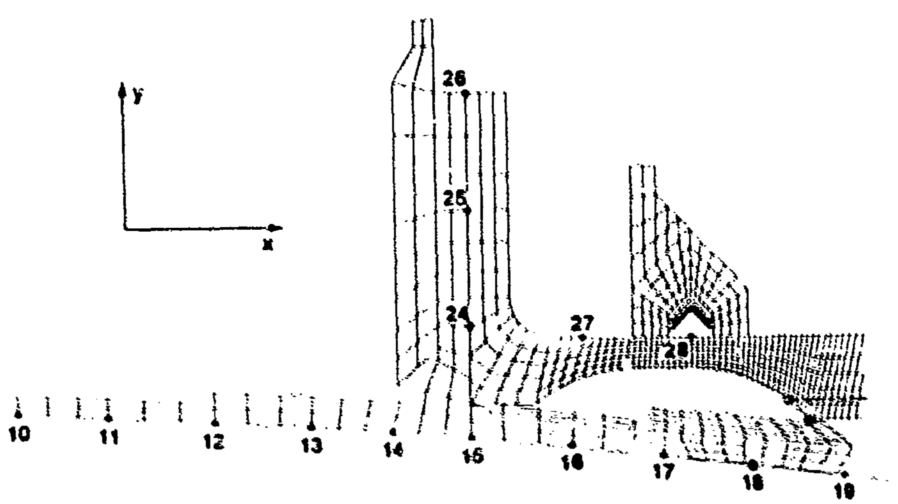


Figure 4 Key and ISA Ring nodes at which velocity histories were saved. The nodes were selected from a larger set of indexed key nodes used in the long-time analysis. The late-time stress analysis calculation is initially driven by the velocity field at these nodes. The radial velocity at key node 18 (circled) was shown to be related to the peak stress at late time. The position of the point on the aft part of the ISA Ring is indicated by boxes for both the ISA Impact and Undercut Ring Calculations.

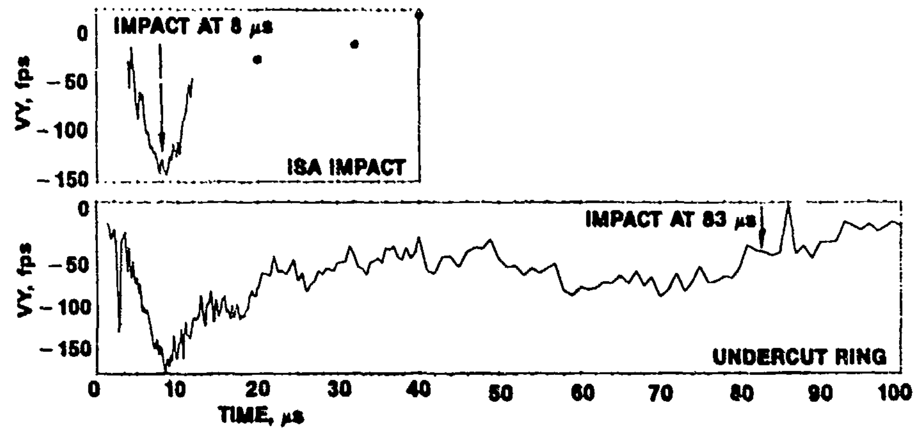


Figure 5 Radial velocities of the point on the aft part of the ISA Ring at the locations indicated by the squares in Figure 4. After 12 μs, only the discrete values indicated by the dots were obtained from the ISA Impact calculations.

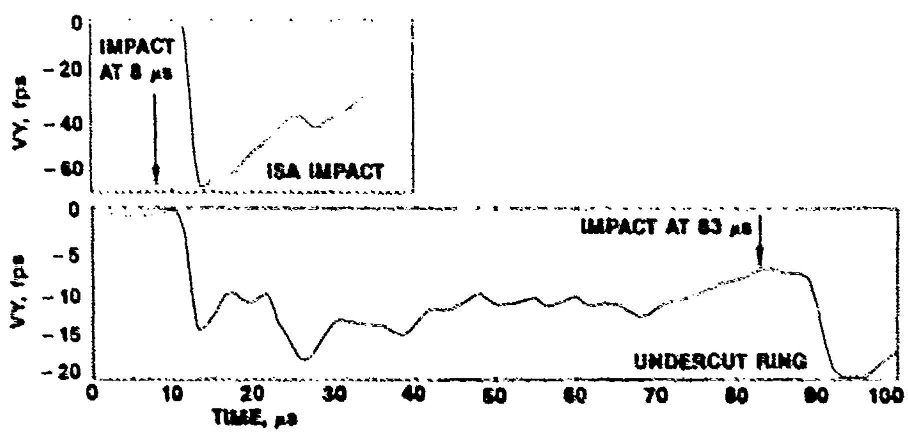


Figure 6 Radial velocity of the tank skin under the Blast Shield at key node 18 indicated by the circle in Figure 4, for the ISA Impact and Undercut Ring calculations. Times of impact of the aft part of the ISA Ring are shown.

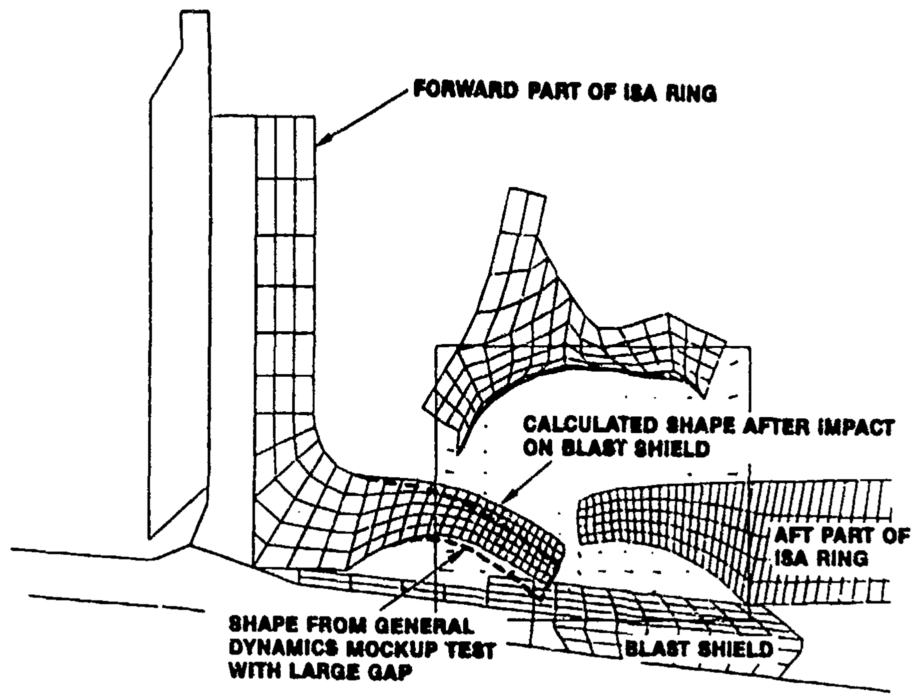


Figure 7 Comparison of the shape of the forward part of the ISA Ring at 20 μs in the ISA Impact calculation to that obtained in a mockup experiment in which the shaped charge was fired through a section of the ISA Ring.

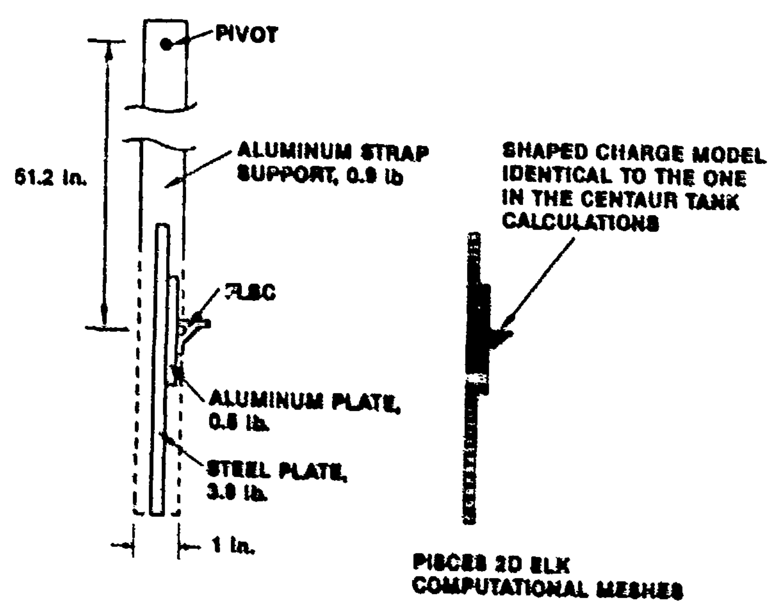


Figure 8 Simple ballistic pendulum for measurement of impulse (momentum) delivered to a structure as a result of detonating the flexible, linear shaped charge (FLSC). The PISCES 2D ELK model of the pendulum mass is shown.

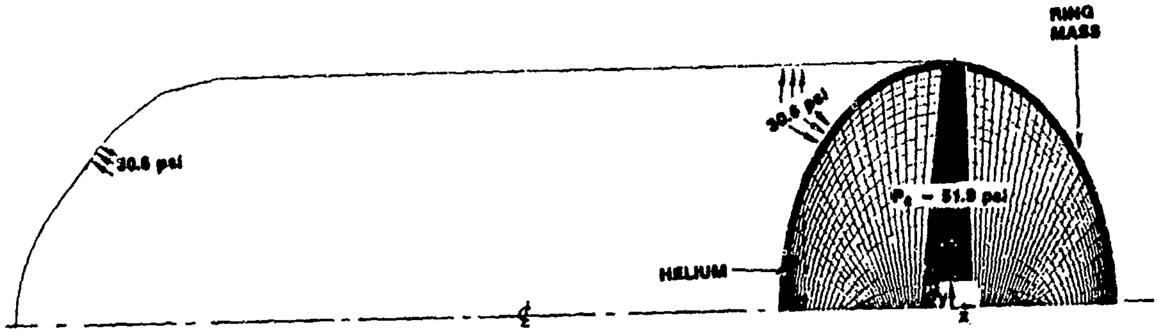


Figure 9 Model of Centaur tanks used in late-time model.

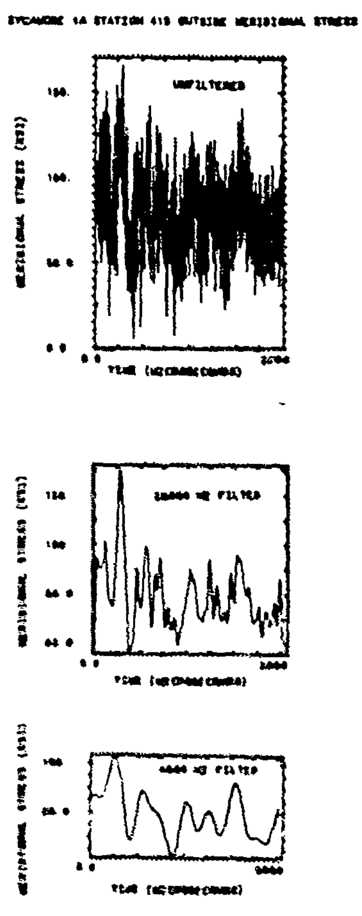


Figure 10 Effect of filtering at station 415 (1 ksi = 6.9 MPa).

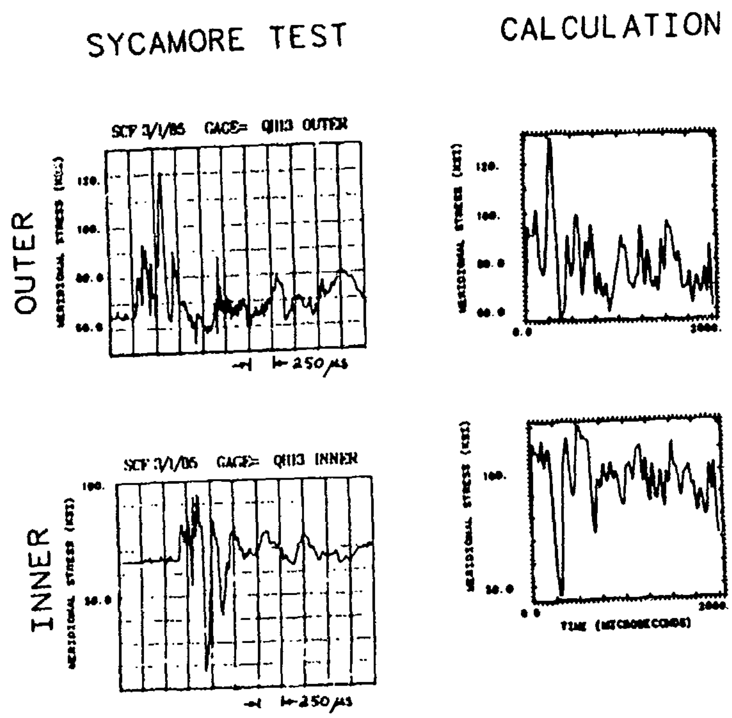


Figure 11 Comparison of meridional stress at gage 3 for AC63 conditions.
(1 ksi = 6.9 MPa).

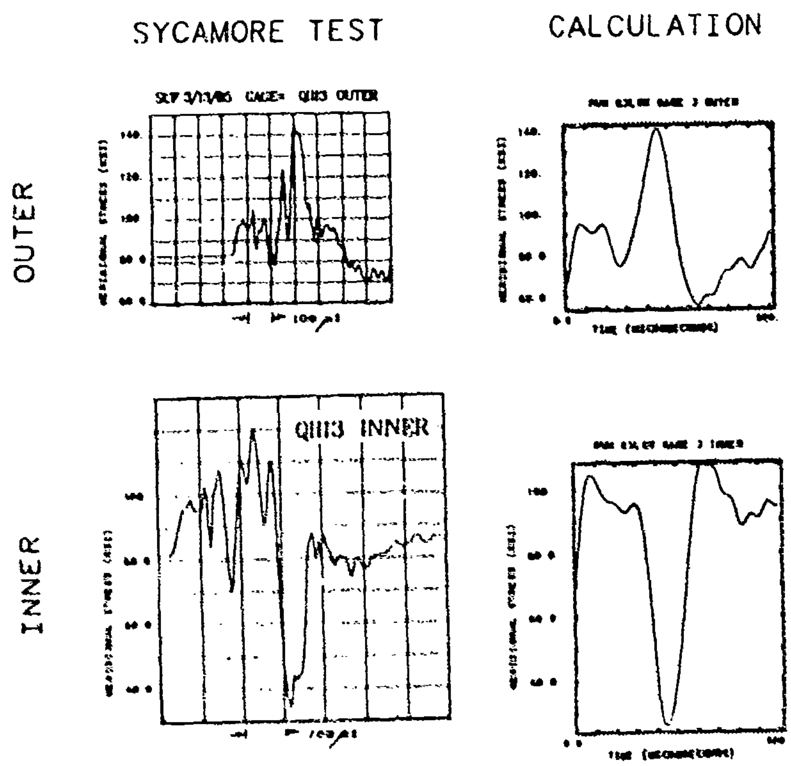
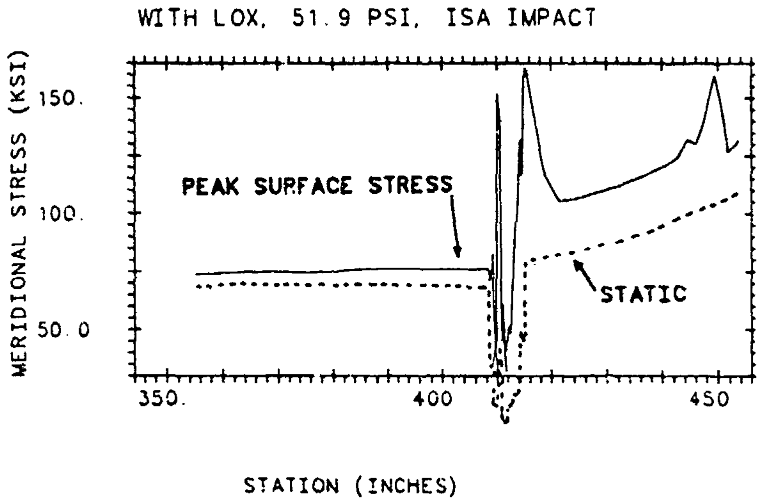


Figure 12 Comparison of meridional stress at gage 3 for AC62 conditions.
(1 ksi = 6.9 MPa).



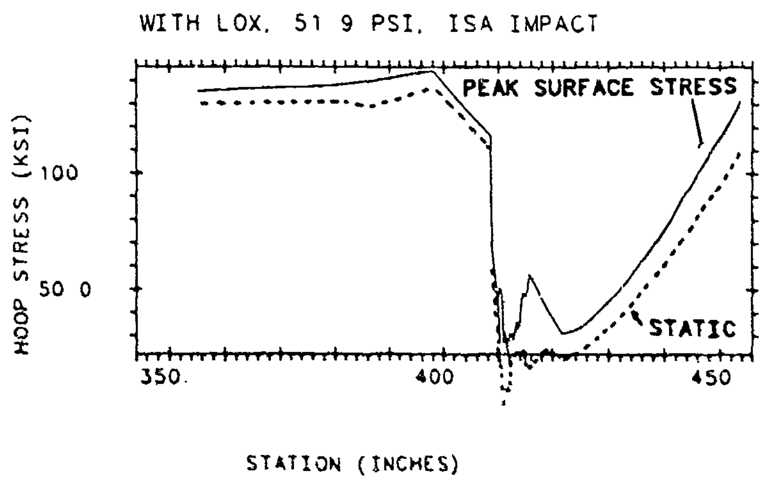


Figure 13 Profile of peak meridional and hoop stresses for AC62 conditions (1 ksi = 6.9 MPa).

PYROTECHNIC SHOCK WORKSHOP

Pyroshock Workshop
Session I
Chairman's Remarks

Daniel L. Van Ert
The Aerospace Corporation
El Segundo, California

This session will be involved with the interpretation of pyroshock data, and the formulation of design and test requirements. Other topics related to pyrotechnic shock will be addressed in two additional sessions this afternoon.

I conducted a survey on pyrotechnic shock prediction techniques some time ago, and I discovered that, by and large, we predict pyrotechnic shock environments by a two step process. First, we define a source level based on previous observations for the type of pyrotechnic device that is involved. Ken Kalbfleisch will present an excellent review of shock sources. Next, we attenuate these source shock levels in accordance with empirically determined loss factors for the path of transmission from the source to the receiver. In most cases we conduct separation tests which are meant to validate those predictions. In my experience, however, the data are seldom studied to gain further insight. Perhaps we don't have the luxury or the motivation to do a more thorough evaluation of the test results. We will hear a paper today by Chuck Moening which examines data from a series of system shock tests to determine more than general conformance with predicted levels. We are dealing with a phenomenon that shows significant event-to-event variation, and that makes the prediction task more dificult. Assessing the accuracy of predictions is complicated by the variability inherent in the shock phenomenon. Often simulation techniques influence the way we develop the criteria. When I worked for the Martin Company, we began to recognize that pyrotechnic shock was an environment to be considered, because some very significant consequences were in evidence. After consulting with our test laboratory, I developed a 160-g terminal-peak sawtooth pulse that I believed to be a suitable pyroshock simulation. It turned out the 160-g pulse was far from appropriate, but had been devised in terms of what I believed to be our test capability. It is difficult to appreciate the significance of the levels involved. As Mr. Moening mentioned earlier, some people assert, "pyrotechnic shock is not even worthy of our consideration." Yet, others overreact when shock levels in the thousands of g's are imposed, because they think of it in terms of steady state acceleration. We think of it as humans and not as mechanical devices. I have seen failures occur in components when they are shock tested at levels that look to be rather insignificant to the eye.

Shock spectrum is a conventional way of specifying test requirements. The presentation by Richard Chalmers will perhaps raise some question about whether acceleration is the appropriate shock descripter. Rounding out this session, Henry Luhrs will present guidance for designing components to withstand pyrotechnic shock. The real value in this session is the interchange we can develop among ourselves, because we have an aggregate of thousands of years of experience sitting in this room. So I encourage free discussion.

PYROSHOCK WORKSHOP SESSION II CHAIRMAN's REMARKS

Glenn Wasz
TRW
San Bernardino, CA

The topics of discussion in this session are instrumentaion, data requirements and databanks. One of the main subjects to be discussed in this session is zero-shift, which will be addressed by three of the speakers.

This session will begin with a presentation on the use of existing database systems to store and to access pyrotechnic shock data. This will be followed by a presentation on determining the special requirements of accelerometers used to make pyrotechnic shock measurements, and to measure other very high amplitude, very short rise time transient shocks. Frequency response, survivability, cabling effects, and mechanical design characteristics of accelerometers will also be included in this presentation. Several presentations will be made on the temporary change in the zero level (zero-shift) of the instrumentation system. These presentations also will include methods for preventing the zero-shift. This will be followed by a presentation on the effects and the desirability of high-pass/low pass filtering both internal and external to the accelerometer. These presentations will be followed by a general discussion at the end of the session.

A VIBROACOUSTIC DATABASE MANAGEMENT SYSTEM AND ITS APPLICATION FOR A PYROSHOCK DATABASE

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The development of a pyroshock database management system is proposed. A candidate data structure for this system is developed from that of a similar system previously configured for vibroacoustic data.

INTRODUCTION

Proposed herein is the development of a pyrotechnic shock database management system utilizing software that was developed for handling a vibroacoustic database system. First discussed will be the advantages of having a pyrotechnic shock database management system, particularly one that is complemented with prediction routines for supporting the development of test requirements. A vibroacoustic database management system named VAPEPS will then be described along with a discussion of how the data structure of this system can be modified to allow for the storage of pyrotechnic shock data. Finally, a brief discussion will be presented concerning an approach for developing analytical and empirical pyroshock predictions.

DATABASE MANAGEMENT

Setting pyrotechnic shock test requirements is an uncertain task. It is usually done empirically with each aerospace contractor using the data sets they have generated, or the data sets with which they are most familiar. Structural parameters are accounted for as their previous experience indicates appropriate. As indicated in Table 1, the existence of a universally available database, formed from data sets contributed by the aerospace community, would provide contractors with a broader data source to use for setting test requirements. It is also a way of distributing new data throughout the community in a timely manner. The shared usage of prediction schemes that have been incorporated into the database software, and updating these prediction schemes when the community, as a whole, thinks appropriate also provides the contractors with the best available prediction procedures.

TABLE 1
Advantages

- PROVIDE PAYLOAD CONTRACTORS WITH A MUCH BROADER DATABASE THAN PREVIOUSLY AVAILABLE
- TIMELY DISTRIBUTION OF NEW DATA THROUGHOUT THE AEROSPACE COMMUNITY
- PROVIDE FOR A WIDE DISTRIBUTION OF BEST AVAILABLE PREDICTION PROCEDURES

A vibroacoustic database was developed to serve as a repository for vibroacoustic data obtained from flight measurements and ground tests of Space Shuttle and expendable booster payloads. It also has environmental prediction software which uses the data in the database (Table 2). The name "VAPEPS" is an acronym from the character string shown in Table 2. Its software is compatible with the computers shown in Table 3, and it has been well documented as a NASA contractor report. That report contains users manuals, a programmer's manual, sample problem manuals, and a technical manual. This activity was sponsored by both NASA and the Air Force. The software is nonproprietary, and it is available from the NASA Goddard Space Flight Center.

TABLE 2
General Background

• DATABASE MANAGEMENT SYSTEM TO SERVE AS A REPOSITORY FOR VIBROACOUSTIC DATA
• VAPEPS (VibroAcoustic Payload Environment Prediction System)
• VAPEPS SOFTWARE INCLUDES SOFTWARE THAT CAN BE USED TO PREDICT THE ENVIRONMENT OF NEW PAYLOADS

TABLE 3
Software compatibility

• VAPEPS SOFTWARE IS COMPATIBLE WITH: — SPERRY (UNIVAC) — DEC VAX — CDC — CRAY 1S — MASSCOMP (UNIX)
• DOCUMENTATION — NASA-CR-166823
• SPONSORSHIP — NASA/GODDARD SPACE FLIGHT CENTER — UNITED STATES AIR FORCE SPACE DIVISION

Briefly, "VAPEPS" has been configured to either serve the needs of a local site, or data may be exchange between local sites as shown in the upper left an right corners of Fig. 1. The data may also be exchanged between one local site and another local site with one of the local sites acting as a global database administrator — this is shown at the bottom of Fig. 1. The latter arrangement is the best way to handle a pyrotechnic shock database, i.e., having a global database administrator distribute data to other user sites.

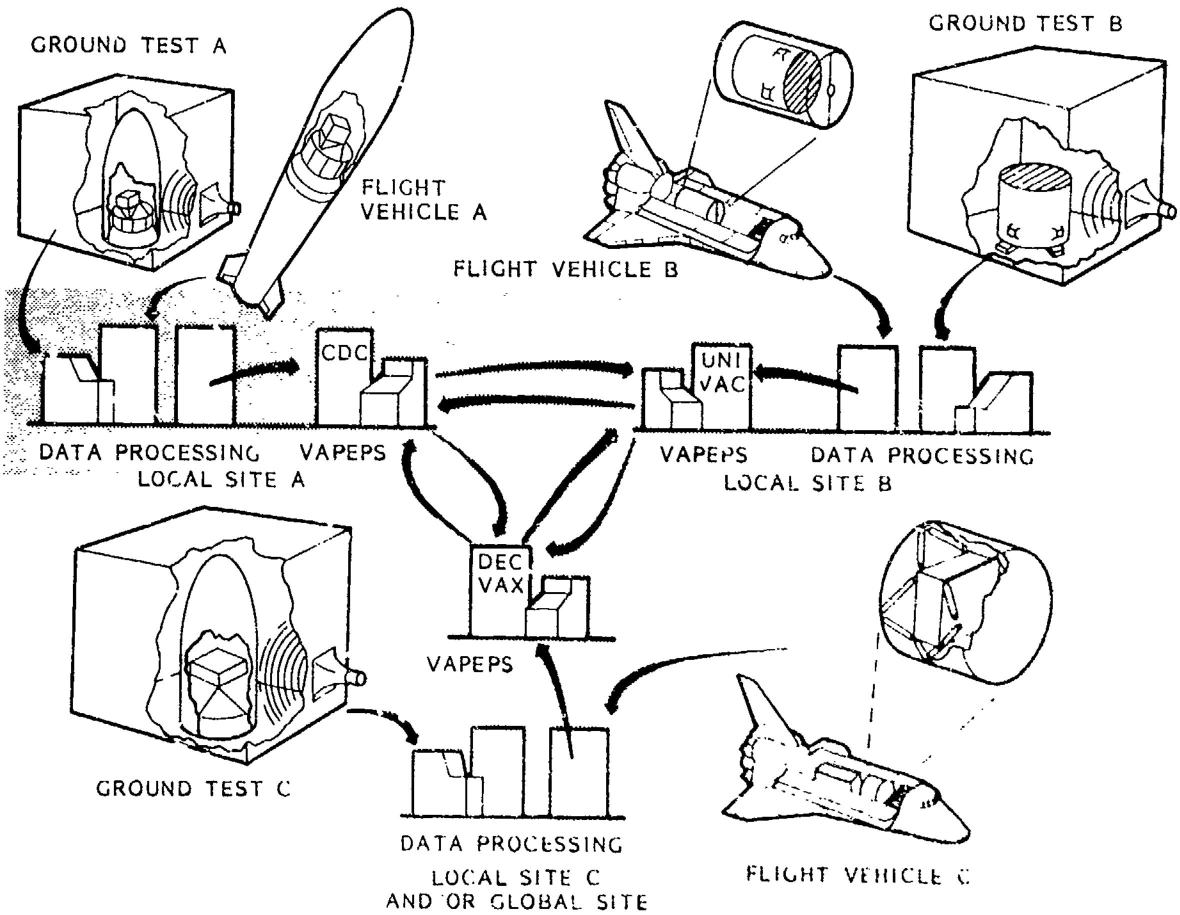

Fig. 1 Local and community shared usage

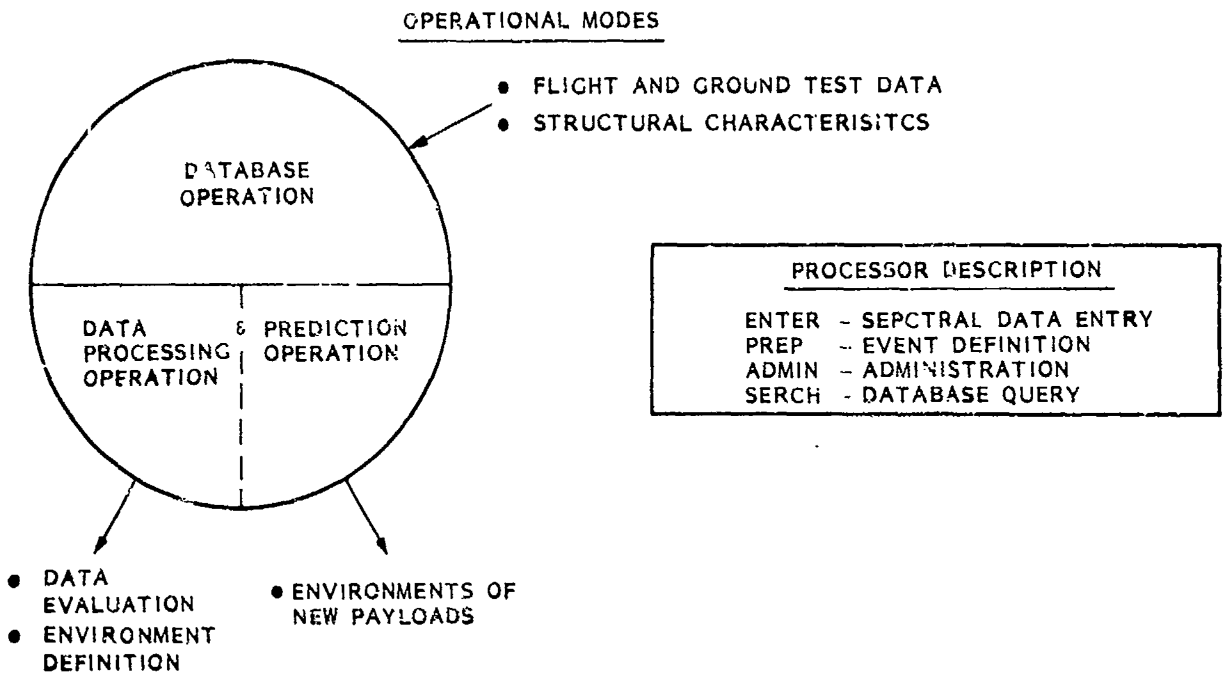

Fig. 2 VAPEPS software architecture

The "VAPEPS" system has two basic operational modes, a database management mode and a data processing and prediction mode. The prediction mode allows the user to make theoretical or empirical predictions using the database. The data processing mode accommodates arithmetical, statistical, logical and various other operations such as matrix manipulations of data sets and power spectral density analyses of amplitude-time series. The database operating mode has four phases, spectral data entry, event definition, administration, and database search and query (Fig. 2). Each phase is controlled by a processor as named on the right hand side of Figure 2.

The "ENTER" processor brings in power spectral density or spectrum level data into the database in any user-defined format. The user only has to identify the analysis filter, units, etc., used to process the data. A similar processor for pyroshock data would have to accommodate shock spectrum and amplitude-time signals. The "PREP" processor consists of many subprocessors which are used to characterize a data set. It provides for constructing configuration trees to describe a payload and also provides for building modules of structural parameters and the associated measured response data. It is these modules that form the basis of empirical prediction schemes. Additional details concerning "PREP" are discussed below. The "ADMIN" processor, (Fig. 2) is for the database administrator, no one other than the administrator can enter data into the system. The administrator's function is to examine the data to ensure the data have been properly classified, and most important, to ensure the validity of the data. Finally, the processor "SERCH" allows retrieval of data with attributes of interest as selected by the database user.

Fig. 3 shows one of the subprocessors that make up "PREP." It is the subprocessor named "BOOK." By calling it the user is prompted to name the agency that processed the data, his contractor and the cognizant government agency, the date/time of the test or flight, the type of event (liftoff, transonic, staging, solar array deployment, ground test, etc.), the location of the event, and the vehicle from which the data was obtained. The location, if a flight, refers to the launch pad, either the Eastern Test Range or the Western Test Range. If the event was a ground test, then the location refers to the site of the ground test. The vehicle refers to whether the payload was flown on the Space Shuttle or on an expendable booster. If the vehicle was the Space Shuttle then the Shuttle is named as the class of the vehicle and the specific Shuttle flown is the type of vehicle. The vehicle configuration names the payload, e.g., Space Telescope, Space Lab, and the like.

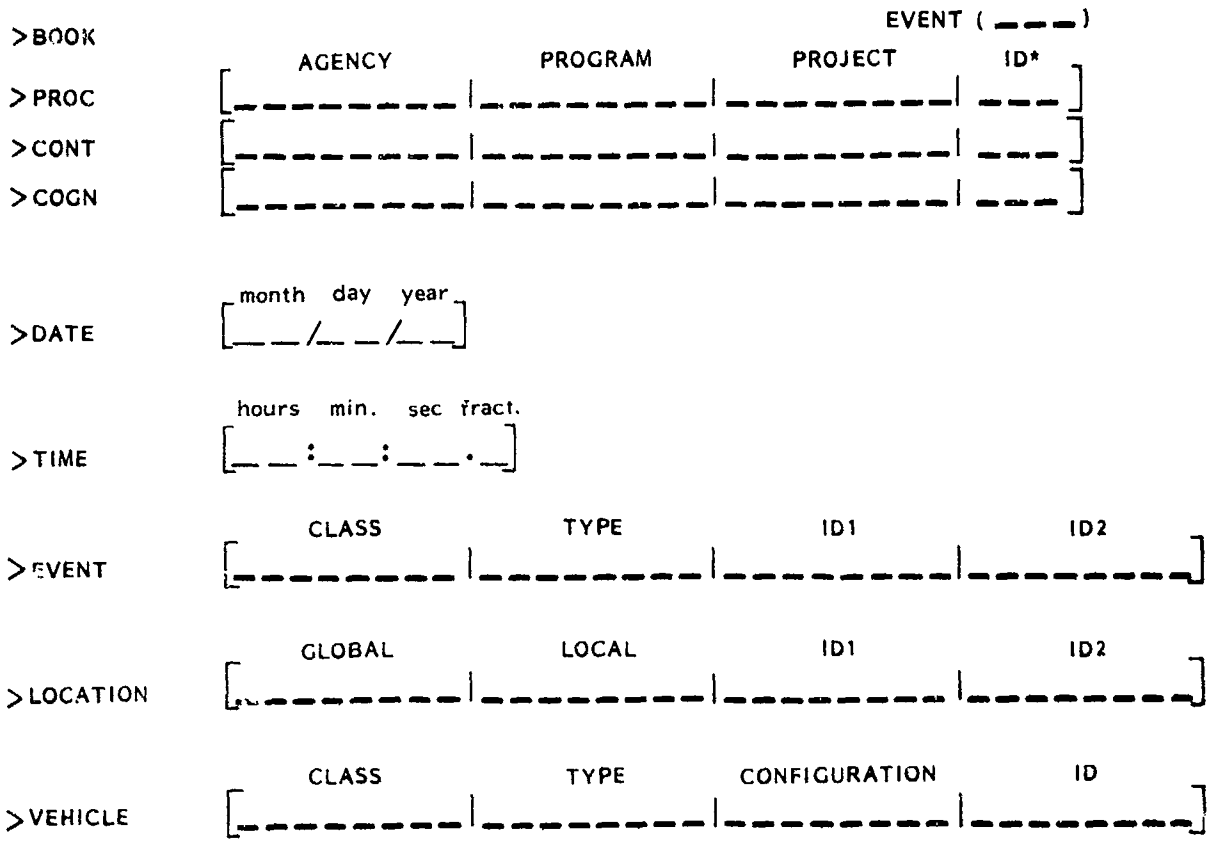

Fig. 3 Subprocessor

Once these data are entered, the processor "SERCH" allows the user to pull out data sets associated with any of the characteristics named in the fields of the arguments of the "BOOK" subprocessor. Several free fields exist for including special data of user interest which for a pyroshock database might indicate the source of the shock, pin puller, separation nut, or the like. Another subprocessor allows the user to build "configuration trees" such as are illustrated in Fig. 4. The user is allowed considerable freedom in the construction of the tree. The Space Shuttle in Fig. 4 for example; it is broken down into the main cabin, the cargo bay, SRB, SSME, the expendable tank (ET), and the OMS pod. The cargo bay is further divided into a DFI pallet and an ESA pallet with its payload. There are 100 different levels that can be employed for detailing a structure.

The "data module" processor illustrated schematically at the left of Fig. 5, pulls together the data needed to perform empirical predictions. The use of this processor is demonstrated for a payload on a ESA pallet. The configuration tree for this payload and pallet is shown at the right of Fig. 5. The payload has been numbered. Each number corresponds to a statistical energy element; statistical energy rationale forms the basis of the prediction scheme. Each element is also labeled. By employing this processor the user is prompted to identify pertinent structural parameters and the measured response data (by identifying channel numbers) associated with each of the labeled statistical energy elements. The "data module" processor allows the user to include such generic information as to whether the structure is a flat panel, a curved panel, or is of honeycomb construction, etc. For pyrotechnic shock, the users may wish to specify the direction of the measurement, lateral or normal, the distance from the source, and the pyrotechnic shock device. Once this data module, is constructed another command "ATTACH" allows the user to associate their data with an appropriate member of the configuration tree.

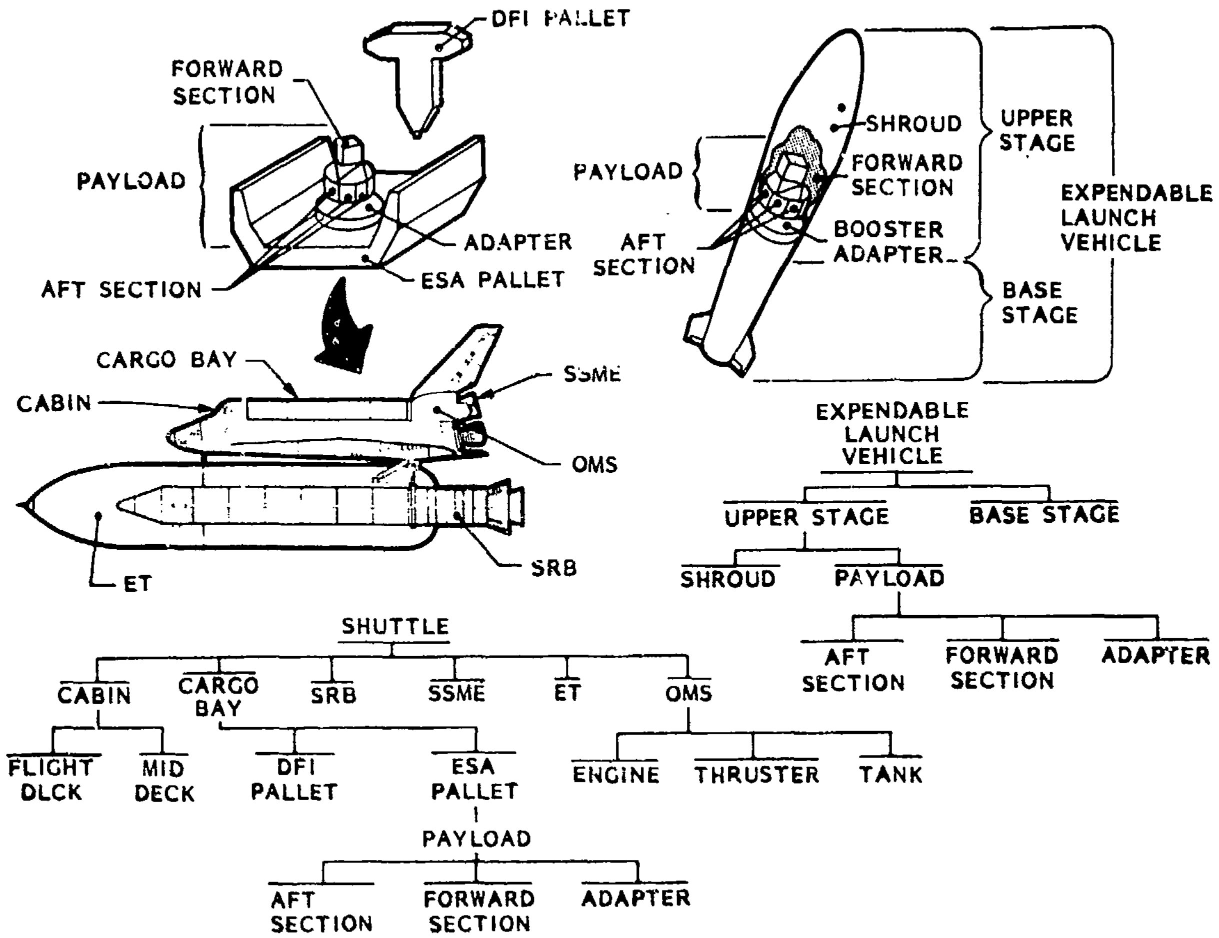

Fig. 4 Configuration tree

Once structural parameters are entered into the database, nondimensional parameters are automatically created using the scales shown in Table 4. These nondimensional parameters allow the database to be searched for dynamically similar data sets on which to base predictions. The search can also include the configuration trees such that dynamically similar data sets are obtained from payload configurations similar to that of a new payload for which an environment expected in flight is to be established.

To develop an analytical pyroshock prediction scheme it is proposed to make use of the statistical energy concept, a number of relatively successful investigations using this approach have already been performed. As previously mentioned the database supports empirical predictions in that users may search for dynamically and configuration-similar data sets having the appropriate pyrotechnic excitation sources (pin puller, separation nuts, etc.). These data sets could then be operated on to make corrections to

TABLE 4
Scales Used in Forming Nondimensional Parameters

• PANEL-LIKE STRUCTURES	
LENGTH SCALE :	$\sqrt{Ap}$
TIME SCALE :	$\sqrt{Ap}/C_l$
FORCE SCALE :	$\rho_\infty ApC_l^2$
• BEAM-LIKE STRUCTURES	
LENGTH SCALE :	B_l
TIME SCALE :	B_l/C_l
FORCE SCALE :	$\rho_\infty B_l^2 C_l^2$
• ACOUSTIC SPACE	
LENGTH SCALE :	$V^{1/3}$
TIME SCALE :	$V^{1/3}/C$
FORCE SCALE :	$\rho V^{2/3} C^2$
• FLUID DYNAMIC PRESSURE FIELD	
LENGTH SCALE :	B_l
TIME SCALE :	B_l/Vel
FORCE SCALE :	$\rho B_l^2 Vel^2$

the constant velocity and acceleration slopes that characterize pyrotechnic shock spectrums. A successful prediction scheme would be one that resulted in conservative but reasonable test requirements.

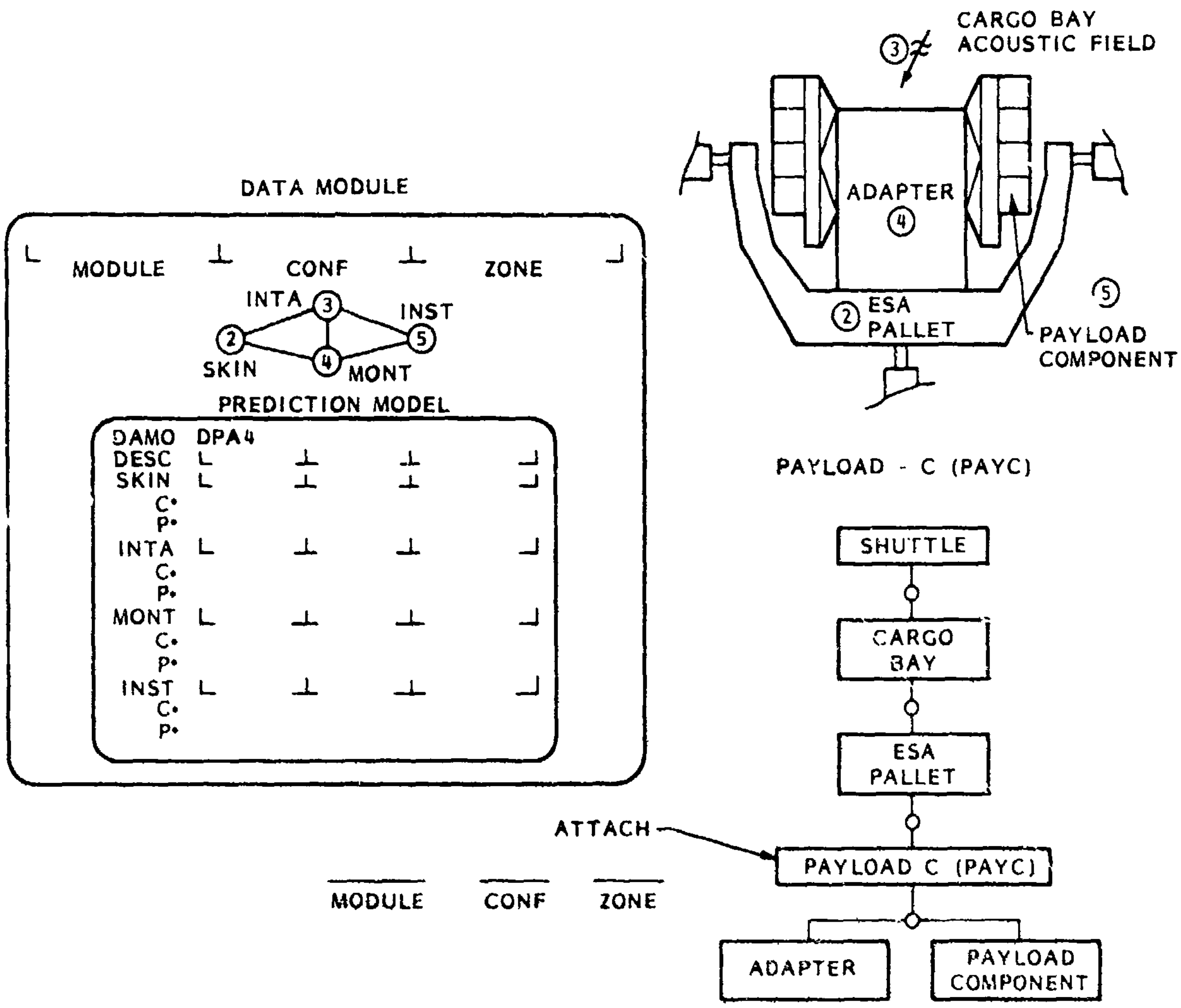

Fig. 5 Data module

DISCUSSION

Mr. Baca (Sandia Laboratories): Is your database system as it is currently configured, stored on disks so it is always on line? It seems as if you have a great deal of data, do you have the space on the disk to handle it?

Mr. Henricks: Yes, it is always on line. The actual database could also be on tape, and then we can bring it on line.

Mr. Baca: How much disk space do you presently have?

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STATE-OF-THE ART ACCELEROMETER CHARACTERISTICS FOR PYROTECHNIC SHOCK MEASUREMENT

Jon Wilson
Consultant
San Juan Capistrano, CA

ABSTRACT

This paper presents a brief history, a brief summary of a user survey, and a brief summary of a manufacturer's survey that was conducted to find the state-of-the-art for pyrotechnic shock measurements. It provides a chart summarizing the characteristics of several different manufacturer's accelerometers.

INTRODUCTION

Last night Dan Powers said he has been coming to these meetings for 20 years. He has heard the same complaints, the same problems, the same discussions, and the same responses from the accelerometer manufacturers. For 10 of those years I was providing some of those manufacturers' responses, so I am quite familiar with the stories. The basic problems have been survivability, zero-shift, and low frequency noise.

USER SURVEY

I conducted a survey of users. I contacted eight users of accelerometers who are currently making pyrotechnic shock measurements, either in-flight, or in the laboratory; so this covers both applications. I asked the users what their current problems were. The problems shown above the line in Figure 1 were given to me by the users; they are survival, zero-shift, low frequency noise and saturation. Until the last few years very little had been done about these problems. There was very little development on the part of the accelerometer manufacturers because of the perceived high investment and probable low return on a limited market. But, the state-of-the-art has begun to change over the last few years. For example, on the problem of the survival of accelerometers, Scott Walton gave a paper yesterday on how he solved his instrument survival problems. Some of his techniques might be applicable to pyrotechnic shock measurements.

On the problem of zero-shift, Anthony Chu gave an excellent paper yesterday on zero-shift, and three presentations in this session will address that problem. Anthony Chu mentioned the low frequency noise and saturation problem in the context of the zero-shift problem, but I think they contribute to other problems as well.

The four problems listed at the bottom of Figure 1 are not necessarily perceived by the user as being a problem; they may be a worse problem, or they may cause some of the problems, shown above the line in Figure 1, that the users are aware of. Fred Shelby, from the Sandia Laboratories, gave a paper on using internal filters to suppress resonances at the 1983 Transducer Workshop. Anthony Chu mentioned this in his paper yesterday, and Endevco is doing this to suppress the resonances to get rid of the resonance problems. Calibration has always been one of my pet concerns. How are pyrotechnic shock accelerometers calibrated? How meaningful are those calibrations? Cable and connector problems are being solved by eliminating the connectors, going to low impedance, and work on cable and connection schemes. Base strain is a problem which will be discussed later, and Anthony Chu mentioned that as a contributor to the zero-shift problem.

Figure 2 is a typical pyrotechnic shock time-history trace; notice the high amplitude negative peaks as well as the very high positive peaks. I will mention later why they may be important in the problems we are having.

The importance of an accelerometer's frequency response and resonance are often overlooked. The top plot in Figure 3 shows a test shock response spectrum specification and the actual shock response spectrum that were generated during that test. The dashed lines are the possibilities of what might be hap-

pening above 10 kHZ, where the test specification ends. The test shock spectrum ends, but the energy does not end. The lower plot in Figure 3 is a possible frequency response curve of an accelerometer. Notice, at the resonant peak the response peaks at 45 dB above the flat portion of the curve. What does that do to the response spectrum if significant energy is present at those high frequencies? I believe significant energy is present at those high frequencies. You need a high resonance frequency accelerometer to get away from, or to reduce, the effects of the accelerometer resonance; or, as in the case of some recent developments, you need some way to suppress that resonance so it doesn't get into the signal.

VENDOR SURVEY

Table 1 shows the characteristics of some accelerometers presently on the market; the information was furnished by the manufacturers. All of the accelerometers were rated for 100,000 g's full scale. I asked each manufacturer what is the zero-shift of your accelerometer? How much zero-shift would I most likely see in a pyrotechnic shock event? Notice the first two manufacturers didn't know the answer to that question. The next two manufacturers' data sheets showed "no zero-shift." The data sheet for Brand E showed "imperceptible" zero-shift. Brand F showed less than 0.1% zero-shift on their data sheet, and both Brands G and H showed "negligible" zero-shift on their data sheets. The initials (S.T.) for the Brand G accelerometer mean it was sample tested; "eval" for the Brand H accelerometer means it has been evaluated by their customers.

For survival: Brand A said, "we test our accelerometers for survival," while Brand B said, "we don't get very many of them back." Brands C and D analyzed their designs to ensure they would survive, and they also did some sample testing. In the case of Brand D their accelerometer was also evaluated to high acceleration levels by a customer, and it was shown to survive. Brands F and G were tested for survivability while the survivability of Brands E and H was unknown.

As to the calibration method, this was one of my concerns when I was trying to sell accelerometers for shock. Most accelerometers were calibrated by using very low level vibration, and Brands A and B were indeed calibrated by using vibration. The rest of the accelerometers were calibrated by shock. The next line, calibration level, shows Brands C, D, and E are calibrated by low level shocks, and the levels were not given. Brand F's accelerometers are calibrated at 10,000 g's, and Brand H's accelerometers are calibrated at 5000 g's.

The frequency response characteristics of most accelerometer brands were not specified. The resonance frequencies varied. The resonance frequency of Brand A was specified at 180 kHz. The resonance frequency of Brand D was suppressed with an integral electronics design internal filter. The resonance frequency of Brand F is 300,000 Hz, and its resonance is also suppressed. Brand G's resonance frequency is greater than 250,000 Hz.

The maximum tolerable transverse motion refers to the manufacturers' specification of the maximum transverse motions their accelerometers can survive. It does not refer to the transverse sensitivity. We are told the transverse motion is approximately equal to the motion in the sensitive direction in pyrotechnic shock tests. The motion is supposedly the same in all directions in pyrotechnic shock. After looking at this data I decided perhaps this is why some "100,000 g" accelerometers break at much lower levels during pyrotechnic shock tests. Some accelerometers have no specification for the maximum allowable transverse motion. Some accelerometers have a 20,000 g or a 30,000 g specification for the maximum allowable transverse motion. The maximum allowable transverse motion for Brand F is 100,000 g's or greater. Likewise the maximum negative shock loading leaves something to be desired in most cases.

Last, but not least, is the problem of base strain. We know from technical papers, and Dan Powers showed us an illustration in this morning's session, of the kind of motion at the mounting surface of a test plate. We know an accelerometer's mounting surface undergoes a great deal of stress and strain, and yet some of the accelerometers shown in Table 1 have fairly high base strain specifications. Some of the other accelerometers have base strain specifications that are difficult for me to believe, because I know how they are designed.

CONCLUSION

To conclude, I think the state-of-the-art of accelerometer characteristics is not yet where it should be, but it seems to have improved over the last few years. The manufacturers are starting to respond to the users' needs, and I hope these comparisons will help both users and manufacturers to see where improvements are needed. I also hope they will stimulate a discussion of

requirements, that is, are some of these characteristics really required? I think Dan Powers had some questions whether the frequency response and the resonance frequency characteristics of accelerometers have to be exotic. I hope this will also encourage the manufacturers to improve their specifications and the information for their customers, because my calls to the manufacturers were similar to a user's preliminary inquiry to a manufacturer, and in many cases, I did not feel I got many good answers. I believe, in some cases, even though this represents the information I got, it may not represent the manufacturers' best performance. I hope the manufacturers who are present can point out areas where this relatively poor performance is really not that bad.

SURVIVAL

ZERO-SHIFT

LOW FREQUENCY NOISE

SATURATION

RESONANCES

CALIBRATION

CABLE/CONNECTOR

BASE STRAIN

Figure 1 - User Problems

DISCUSSION

Mr. Rubin (The Aerospace Corporation) Were all of the accelerometers piezoelectric?

Mr. Wilson: No, they were not. Some accelerometers were piezo-electric and other accelerometers were piezoresistive. Some accelerometers had internal electronics while others were high output impedance charge type devices.

Mr. Maier (Endevco): Of the specifications you showed, are there others that should be included? Also, has any thought been given to an acceptable limit to zero-shift?

Mr. Wilson: I think you should ask the users if any other specifications should be included. As far as acceptable zero-shift is concerned, I prefer to leave that to the other three speakers.

TABLE 1

PYROSHOCK ACCELEROMETER
CHARACTERISTICS SUMMARY

BRAND	A	B	C	D	E	F	G	H
FULL SCALE, g	100 k	100 k	100 k	100 k	100 k	100 k	100 k	100 k
ZERO SHIFT	?	?	Sp. = "None"	Sp. = "None"	Sp. = "IMP"	Sp. "/0.1%" (S.T.)	Neg. (S.T.)	Neg. (Eval.)
SURVIVAL	Tested	?	ANAL. & S.T.	ANAL. & S.T. (Eval.)	?	Tested	Tested	?
CAL. METH.	Vib.	Vib.	Shock	Shock	Shock	Shock	Shock	Shock
CAL. LEVEL	10g	"Low"	"Low"	"Low"	"Low"	1 Kg	10 Kg	5 Kg
FREQ. RESP. ± 5%, kHz	39	12	8	> 25 K (Eval.)	N.S.	50 (± 1dB)	180	15 (± 10%)
RESONANCE, kHz	Sp. 180	Sp. 60	Sp. 60	Sp. 60 SUPP.	Sp. 100	Sp. 300 SUPP.	Sp. >250	80
MAX. TRANS., kg	N.S.	N.S.	30	30	20	100	>100	N.S.
MAX. NEG., kg	100	60	N.S.	N.S.	N.S.	>100	>150	20
BASE STR., EQ. G AT 250 μG	50	N.S.	50	50	N.S.	< 250	<100	8

Sp. = Specification
S.T. = Sample Tests
Eval. = Customer Evaluations
Neg. = Negligible

N.S. = Not Specified
? = No Information
IMP = Imperceptible
SUPP. = Suppressed

Sources: Manufacturer's Data Sheets, Telephone Inquiries, Published Technical Papers

Compiled By Jon S. Wilson
October 1985

Figure 2 - Pyroshock Time History

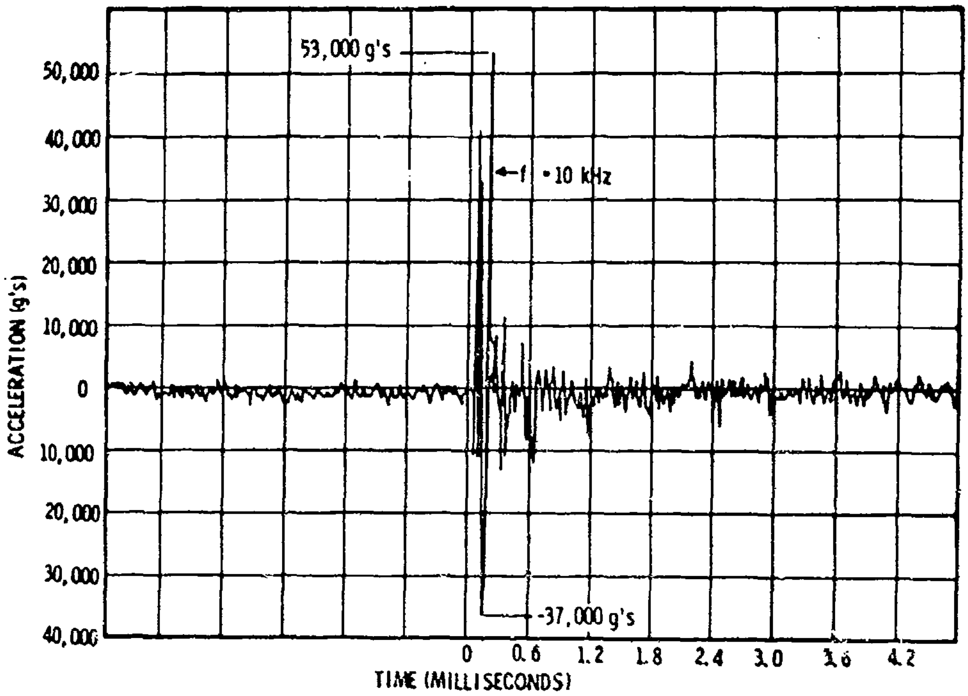
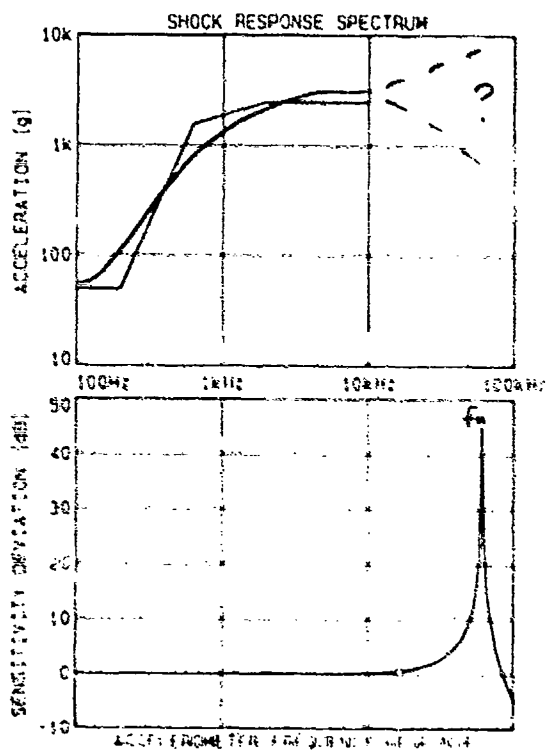


Figure 3

FREQUENCY RESPONSE COMPARISON



Discussion

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Mr. Wilson: No, they were not. Some accelerometers were piezo-electric and other accelerometers were piezo-resistive. Some accelerometers had internal electronics while others were high output impedance charge type devices.

Mr. Meyer (Endevco): Of the specifications your showed are there others that should be included? Also, has any thought been given to an acceptable limit to zero-shift?

Mr. Wilson: I think you should ask the users if any other specifications should be included. As far as zero-shift is concerned I prefer to leave that to the three speakers.

ZERO-SHIFTED ACCELEROMETER OUTPUTS

Arnold Galef
TRW
Redondo Beach, California

In this presentation it is claimed that the commonly appearing zero-shift in Pyro Shock data is usually a symptom of a malfunctioning measurement system, so that the data can not be "repaired" (by high-pass filtering or equivalent) unless tests can be devised that permit the demonstration that the system is operating in a linear mode in all respects other than the shift. The likely cause of the zero-shift and its prevention are discussed.

The first two figures are presented in order to make clear the phenomenon we are discussing. These are accelerometer traces recorded during a pyrotechnic separation of a missile from its mount as a part of the launch sequence.

The first figure is a very typical zero-shift, of the type to be discussed today. (The "double shock" present on the trace is not an aspect of the problem nor is it a symptom of any other instrumentation malfunction. This was measured during a redundant separation. A joint was blown with a shaped charge and another was blown a few milliseconds later to ensure that we don't stay in the hole. Two separate events have occurred). The accelerometer used for this measurement was piezoelectric with integral charge amplifier, and was ranged more than 20db higher than the highest apparent acceleration seen on the trace. This ranging may make it appear difficult to attribute the zero-shift to amplifier overload, as I will. In fact, I believe that the most likely cause of the shift is a saturation caused by the very intense but exceedingly short duration "Pre-Pulse" which is discussed in greater detail in the paper* appearing elsewhere in these proceedings.

Before subjecting the first figure to further discussion it will be useful to examine Fig. 2. This trace is from an accelerometer nearby that of Fig. 1, but ranged much lower. It was intended that this instrument measure the vibration that was expected to occur immediately after the shock. The shock of thousands of g peak-magnitude caused the expected saturation of an instrument ranged at 250g. This completely expected instrumentation system malfunction is not the sort of thing that works its way into data/bases, because everyone recognizes that the type of time history present during the first .5 seconds of Fig. 2 is pure junk, and we had better not treat this as a shock and subject it to detailed spectral analysis. Counterparts of Fig. 1, however, quite frequently find their way into data bases. But I am convinced that the phenomena of Fig. 1, 2 are essentially the same with the difference being one of degree and not of kind. This hypothesizes that, in Fig. 1, the amplifier is operating in a quasi-saturated mode. The next speaker will present six or seven possible causes of Fig. 1 type behavior, including amplifier saturation; I feel that the amplifier is the most frequent culprit.

But, upon examining all of these possible causes of zero-shift, it appears that it is not necessary at this time to identify the explicit cause in any particular case of judging the validity of shock data. All the postulated causes imply that the zero-shift is an indication that the system is not operating linearly so that the data should be discarded!

The above adamant position is not universally accepted, although there is probably universal agreement that the principal spectral symptom of zero-shift, a nearly horizontal shock spectrum at implausibly high levels at low to medium frequencies, is nonsense and should be ignored. However, it is believed by many that-

- The spectral levels at frequencies well removed from the region of the near-horizontal spectrum are nearly correct, so the high level, high frequency portions of the spectra are valid without further processing.

* Galef, A.E. "The Pre-Pulse in Pyro-Shock Measurement and Analysis". Proceedings, 56th Shock and Vibration Symposium

* The zero-shifted data may be corrected by removing the bias (using a high-pass filter or curve-fitting technique) and the subsequent shock analysis is valid, so that if the time history has been retained the entire correct shock spectrum can be generated.

Both of these optimistic viewpoints may be valid in some cases. However, I hold that in order to justify their application in the presence of a significant zero-shift it would be necessary to show that, for the specific accelerometer and amplifier used to take the data, the zero-shift represents the entire malfunction and is not merely a symptom of some other malfunction (specifically, measurement system behaving significantly non-linearly).

Since there have been no causes of zero-shift in a piezoelectric measurement system yet proposed which do not imply a high probability that the shift is merely the most apparent symptom of a malfunctioning measurement system, it appears only reasonable to insist that the burden of proof remain in the lap of the optimist who would like to salvage some or all of the data. And, it should be clear that a successful demonstration of the validity of "corrected" data for a particular case should not be generalized to cases other than that for which the ad hoc linearity investigation was performed since any individual case undoubtedly has its individual peculiarities.

An obvious corollary to the proposition that zero-shifted data may not be repaired in a post-processing procedure unless the cause of the shift is well understood, is that data must not be taken in a manner that would hide the zero-shift if it had had the tendency to appear. But I know of test laboratories of two highly respected companies where a high-pass filter is routinely used in the data acquisition system for the specific purpose of avoiding (more properly, concealing!) a zero shift. A major accelerometer manufacturer has recently introduced a line of transducers with integral charge amplifiers that incorporate high-pass filters for the same purpose. If these people are doing something that is technically inappropriate, as I believe they are, they are certainly doing it in all innocence and in the belief that after the symptom is hidden the patient is well. And, of course, they may be right. But I repeat that the burden of proof is theirs.

We will now leave an annoyingly high fraction of the data presently contained in major documents such as NASA CR 116437 in limbo, and address the problems of avoiding questionable or wrong data in the future. We will largely have to lead each other on this, for the instrument manufacturers are generally much less helpful on this than we might have hoped. I call attention to the Paper by Jon Wilson, presented at this session, where Wilson reports on asking eight major accelerometer manufacturers regarding their zero shift characteristics, and found that two of them were unable to say anything and the others, while claiming superb performance ("none", "negligible" or .1%) were unable to provide test data to support their claims. In the manufacturer's defense it must be conceded that performing quantitative tests is certainly difficult, with the difficulties exacerbated by the frequent presence of the essentially unmeasureable "Pre-Pulse". Their task is complicated still further by the likelihood that in some cases, the rate of change of acceleration is as important or more so than the magnitude, so that information attained by applying a half-sine pulse would be misleading for an equal peak magnitude versed sine and grossly in error for a sawtooth. But these comments clearly suggest that a promising approach requires sacrifice of the ability to measure all frequency components of the shock, and this approach has been recognized for some time by the manufacturers who make either mechanical or electronic low-pass filters available.

I have had good (but limited) experience with commercially available mechanical filters. I can't recommend them unequivocally, however, both because they are not being made for all designs of accelerometers* (they are not usable even with some of the accelerometers made by the manufacturers of the filters!) and because it is clear that there must be a level of acceleration above which the filter is significantly non-linear but the manufacturer has not been able to commit himself (at least to me) where that level begins.

The very good dynamic range of most piezoelectric materials makes feasible an electronic filter that will minimize the very high frequency components of charge generated in the absence of a mechanical filter without significantly affecting the low and middle frequencies of greatest concern. I have again had good but limited experience using the filters of one manufacturer, but have learned to anticipate resistance to their use by many test engineers, who don't like the sensitivity to cable length of the frequency response of the resultant system with filter between the transducer and charge converter stage. It will seem to many that this is a regression to the days prior to development of the charge amplifier, when cable length was a major factor in system sensitivity, but if that is a price that must be paid to achieve valid data then people should prepare themselves to pay it!

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* I would like to caution people against trying to make a home-made filter, either to deal with the unavailability of production devices or to save money. A filter of this type is in principle very simple but in fact is hard to make successfully.

A very rational response to the cable length problem is the elimination of cable by incorporating the amplifier with filter in the accelerometer. Several manufacturers are now supplying such instruments, and although I have not yet had experience with them I am optimistic. (However, as previously noted, I am concerned about one manufacturers inclusion of a high-pass filter as well as the desirable low-pass filter.) For extreme environments an accelerometer of this type mounted upon a mechanical isolator may be optimum.

I will caution all against trying to solve the zero-shift problem by the brute-force approach of increasing the ranging sufficiently to avoid saturation. First, users should be reminded that this can't possibly help if the attempted range change involves using the same transducer and charge amplifier with a higher range setting on the control, since the range adjustment affects the output amplification whereas the potential saturation occurs in the charge converter, or input stage. When this error is avoided, however, the problem of finite dynamic range remains and a range sufficient to avoid saturation will often prohibit data at frequencies below 500 Hz, approximately, that is usefully above the noise level.

In closing, and as a reminder that we may never be sure in all cases of what causes the zero-shift and of how to prevent it, I offer Fig. 3, from the same test that provided the previous figures. Note that the accelerogram provides no obvious indication of malfunction until well after the shocks have been sensed! When we understand this we may understand other phenomena as well. I solicit your comments.

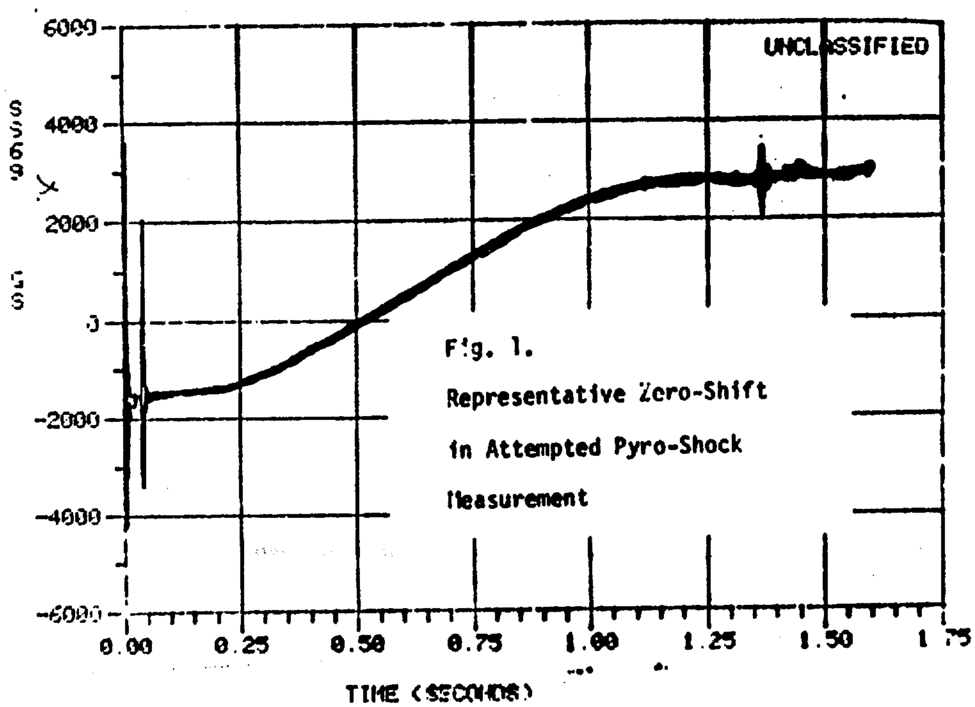

Fig. 1.

Representative Zero-Shift in Attempted Pyro-Shock Measurement

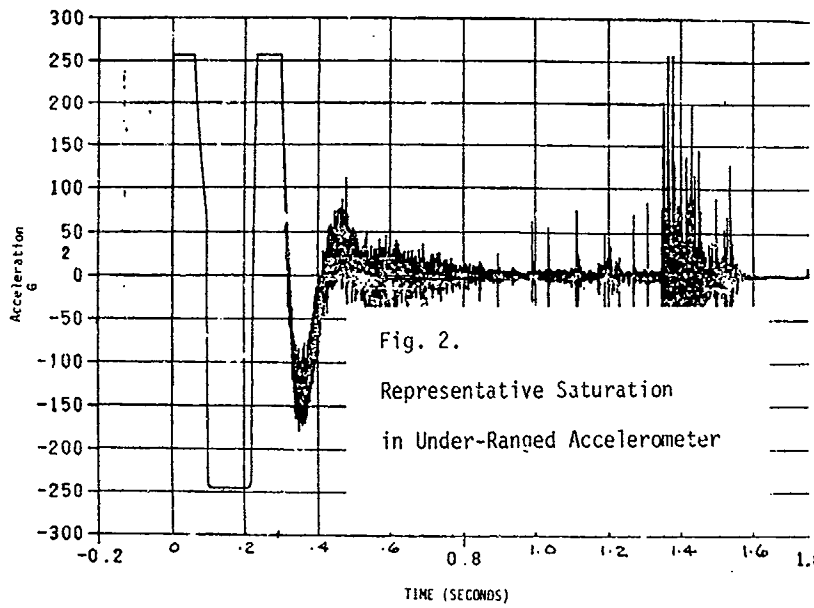

Fig. 2.

Representative Saturation

in Under-Ranged Accelerometer

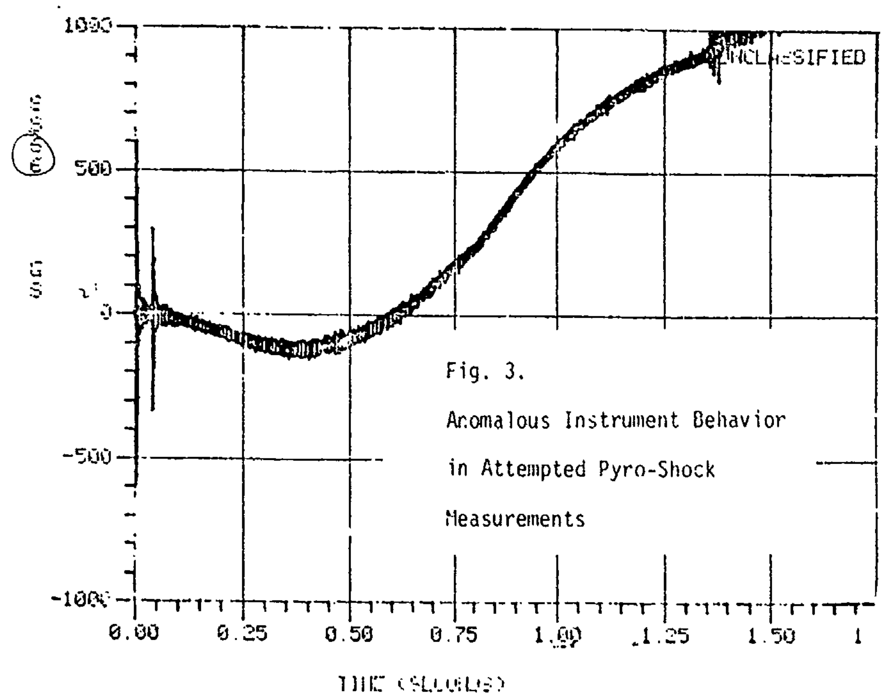

Fig. 3.

Anomalous Instrument Behavior

in Attempted Pyro-Shock

Measurements

QUESTIONABLE EFFECTS OF SHOCK DATA FILTERING

Paul R. Strauss
Rocketdyne Division of Rockwell International
Canoga Park, California

Measurements of Pryotechnic Shock events are unpredictable and often unexplainable. Filtering of accelerometer signals is used and misused in processing the data. This paper is an attempt to show the effect of some filtering processes. A sample case of an electrically simulated measurement is presented to exemplify the effect of filtering on the Shock Response Spectrum, the final data product.

Pyrotechnic Shock is a complex environment. Inadequate devices for measurement of this environment add to the complexity. Because of problems with the measurement system, manipulation of raw and processed data is often performed. The term "manipulation" in this case refers to various types of filtering that are used to make the data fit our needs.

Is filtering necessary? Does filtering cause distortion? It appears that the answers to both questions are "yes." All shock data is filtered whether filtering is desired or not. Filtering is an inherent product of recording, digitizing and amplification systems. Filtering does distort the data; often far beyond the known, or assumed, transfer function of that filter. Filtering is said to be necessary because of the zero-shift apparent in most Pyro shock measurements. It appears necessary to do something to the data to determine what portion of the data is real and what portion is not. But, filtering does not show us what is real, it only changes the data to something that looks real or useful.

The manipulations performed on shock data sometimes go beyond necessity; it is usually an attempt to make the data fit our requirements. We are using low-pass and high-pass filters. In our digital recording systems, we use anti-aliasing filters. Our tape recorder is effectively a low-pass filter. All of our instrumentation is limited; it is filtering the data; and it is biasing the data. Our accelerometer's response also has a filtering effect on the data.

The questions are: What portion of the data is real; what portion is distorted; how we distinguish between the two; and whether or not we can retrieve useful information from it. My main concern is that the data can be manipulated into whatever shape the customer has requested in the shock spectrum.

As an example, I would like to expound on the distortion caused by use of a high-pass filter. This is often used to "eliminate" the zero-shift from time-history data.

Figure 1 is a typical shock time history, typical of the type of data that I receive. A definite zero-shift is apparent immediately after the initial high amplitude pulse. The corresponding shock response spectrum shows a flattening of the low frequency area. On this particular set of data, a vertical line was cleverly drawn, explaining that everything to the left of the line is noise, and everything to the right is data. The diagonal line shows the spectrum we were trying to meet in this particular case. We have basically said that everything above that diagonal line and, therefore, not in "spec," will be noise. Everything that is in "spec" will be data.

This is not necessarily being used to our advantage. To get away from just blindly saying "This is good, and that is not good," we have come up with a filter. We insert a high-pass filter at 20 Hz, and the data looks better (Figure 2). The shock response spectra shows a hump in the curve that wasn't there before. It appears to be indicative of a resonance but is really a response of the filter.

Figure 3 shows the same data with a 200 Hz high-pass filter. According to the input requirement, we are right where we are supposed to be. Therefore, the decision is made that 200 Hz is the proper filter to use. We are in "spec," and everyone is happy.

We decided to play directly with the electronics since, mechanically, we knew that some of the data cannot be real, but we didn't know exactly what to do with it. Most of these conditions were easily simulated in the electronics lab. A time history of a damped sine transient which closely simulates some of the real pulses was set up electrically. A reasonable shock response spectrum is easily produced from it. Figure 4 is a 2,500 Hz pure damped sine wave with the corresponding SRS.

Figure 5 shows what we felt was a true zero-shift. Starting at zero, a steep slope is induced followed by a 25 millisecond decay, returning to zero. Below it is shown the shock response spectrum of that pulse. We have added them together in Figure 6, which appears to be a very good representation of a raw pyro shock measurement. The time history exhibits motion away from zero, followed by high-frequency ringing and the decay to zero. The corresponding shock response spectrum shows a very flat low-frequency region.

Figure 7 shows the same pulse, but with the addition of a 200 Hz high-pass filter. This procedure simply shifted part of the time history such that the signal is now centered around zero. Apparently we have, inadvertently, added a sinusoid, which is one of the effects of the filtering. Just by putting in a 200 Hz high-pass filter, we get a sinusoid that was not there before. This causes the added "hump" in the shock response spectrum. Figure 8 shows this same time history but with the removal of the signal prior to the initial pulse. The shock response spectrum looks slightly better in the 200 Hz area.

Does filtering work? Like any good medicine, we must examine the side effects. We saw that by adding zero-shift to damped sine transient signal, we can simulate a signal which looks like typical test data. What happens when we subtract a zero-shift from our data? In Figure 9, Curve C is the SRS of our simulated distorted shock signal from Figure 6; B is the SRS of the zero-shift from Figure 5. Subtracting B from C does <u>not</u> yield A, the pure damped sine of Figure 4. No combination of digital or analog filtering will free a shifted signal from the distortion. The data is distorted when it exhibits a zero-shift. The effect may be only in the first 100 Hz, it may be in the first 200 Hz. It depends on the system. It depends on the accelerometer, on the range, on the proximity to the charge, and it depends on the instrumentation. Filtering by itself, the way we are using it today in most of our test labs, is not giving us a clean signal and a clean idea of what is really happening mechanically.

Again, I mentioned I am not presenting answers. I am presenting further questions along the same line. We don't have a direct answer to the question as far as how much of the data is good. If an SRS exhibits high levels at low frequencies, that in itself doesn't necessarily mean the data is distorted. I have performed one test where the shock simulator had a resonance at a low frequency. It fooled us. We broke hardware because we assumed the data were not real. There was a resonance in the low frequency of the input device that was causing a low frequency increase in the SRS surve. So you must be very careful. You must examine the data carefully, both filtered and unfiltered. Hopefully, there will be greater understanding and a solution to this with new type accelerometers or new instrumentation in the near future.

DISCUSSION

<u>Mr. Kalbfleisch (TRW)</u>: Before you can entertain processing the data with filtering, you have to make an assumption that the sensitivities and the calibration remain consistent. If it is a piezo-electric accelerometer, during the zero shift your pico-coulomb per g calibration factor remains constant, or the amplifier calibrations continue to remain constant. What data do you have to support that you can continue to use the data, and attempt to process it to remove the zero shift? Or, do we subscribe to Arnold Galef's theory that says it is a nonlinear situation? If there is zero shift, we just discard it completely.

<u>Mr. Strauss (Rocketdyne)</u>: We cannot afford to discard all distorted data but I certainly agree with that. Once we know that the signal is distorted we must label it as distorted or filtered, whichever is applicable.

<u>Mr. Kalbfleisch</u>: Are you attempting to process and use a distorted signal?

<u>Mr. Strauss</u>: That is correct. And today we are using information from that signal. We know it is distorted, but we are still using it.

<u>Mr. Kalbfleisch</u>: So the questions really are can we do that? Is the calibration correct, and can we process that data? What guidelines does one have to attempt to process the distorted signal versus completely discarding anything with any evidence of zero shift?

<u>Mr. Strauss</u>: I guess one way to look at this is by assuming that the distortion comes from

the accelerometer itself; if we have a piezo-electric accelerometer, if we know there is no permanent damage, if we know there is no motion of the crystal, then maybe we can say yes, the calibration is still adequate. On the other hand, if we assume that distortion comes from the amplifier, maybe we can use one part of the data where we know the amplifier is stable.

Mr. Chu (Endevco): That depends on what causes a zero shift. If it is the crystal problem, at that instant that you have zero shift, some of your crystal is depolarized and the sensitivity is not the same at that particular moment; you don't have the same calibration. But if the zero shift is caused by base-strain, or some other factors, then your crystal is still putting out what is seen. At that instant, maybe your calibration is still valid. But as to how to offset or how to justify a zero-shift, how many g's that is, no, I don't have an answer.

Mr. Kalbfleisch. It sounds as if it is also the charge converter or the amplifier, then those data are very suspect. It would be very difficult to assume that we could use data with any evidence of zero-shift.

Mr. Strauss: That is correct. Until we can define the exact source of the shift, then we can't really say for sure that any of the data are good.

Mr. Kalbfleisch: Can we ever define that?

Mr. Strauss: I believe that we will in the near future. But obviously we can't define today where these particular things are coming from.

Mr. Favour (Boeing Aerospace Company): You mentioned you had a high-pass filter, and you injected the sine wave. I would guess that you had a high-pass filter of a box-car function. If you take the inverse transform of that, you get $\frac{\text{sine } X}{X}$, and you get exactly what you expect there. In our experience for any transient work like this, you should use a Bessel, A Gaussian or a linear phase filter, as against the Butterworth filter, because they produce far less ringing. The Butterworth filter is maximally flat, but it doesn't have the best phase characteristics. We found over fifteen years ago you can get some DC bias in your data depending on the quantization intelligence of your A/D converter. For instance, what jumped up and bit us back around 1970 was our A/D converter would quantize by chopping to the negative full scale direction. This, in effect, put a half a bit bias on all data. When we took a Fourier transform of that, we ended up with low-frequency aberrations, somewhat similar, but not as severe as the zero-shift that you are seeing here. We were able to remove that and we did it statistically. But also, if you go to a higher and higher word size, like to a fourteen and sixteen bit word size, obviously the bias diminished, and the problem went away. It is somewhat insidious, and you really have to pay attention to what is going on in your digital electronics.

Mr. Strauss: Thank you. We have now added one more possibility as far as the source of this is concerned. As far as the filtering, I agree with you.

We try to represent here the type of data, the type of filtering techniques, and the types of responses that we are seeing in data. I agree that there are certain types of filters that do minimize the distortion. It all has to be watched very closely.

Mr. Sill (Endevco): We use a two pole Butterworth filter with our piezo-electric accelerometers. The reason we use that is we looked at some of the work being done at Sandia. I think Pat Walter had a paper on different kinds of filters: Chebychev, Bessel, and Butterworth. He favored the Butterworth because of all the combined factors.

FIGURES NOT AVAILABLE AT TIME OF RELEASE;

AVAILABLE FROM AUTHOR

COMPARISON OF RESPONSE FROM DIFFERENT RESONANT PLATE SIMULATION TECHNIQUES

Robert E. Morse
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I will talk about two applications of one technique, the "Resonant Plate" technique, which you have heard other speakers talk about earlier. It is not a method that I developed. The two systems that I will describe have been developed by two different test labs. One was developed by the TRW test lab, and I think Don Pugh gets the biggest portion of the credit for developing our "Resonant Plate" technique. The other one was developed at Lockheed. They did a very good job on their "Resonant Plate" system; we are presently using it on a program I am involved with now.

I want to give credit to several people for Figure 1 which essentially depicts the Resonant Plate system in terms that we all should understand. This is essentially what we are trying to accomplish with the Resonant Plate system.

The first plate is one we developed at TRW with a longitudinal impact. The shock response spectrum requirement that we had to meet was the same in all three axes so the spectrum for our component was the same in all three axes. The spectrum had fairly tight tolerances on it, but we got some relaxation later. We had a major problem in the low frequencies where we were out of the dynamic range of the measurement system, and it took a great deal of work to convince the customer, and even some of our colleagues, that the data were poor and the reason was that the measured data were out of the instrumentation dynamic range. We were getting about a 4,000-g peak response spectrum. We mounted the specimen in three separate orientations on this plate.

Figure 2 shows the response spectrum that we were required to meet. It peaks up to 4,000 g's at about 3,500 Hz.

Figure 3 shows the plate we developed for this response spectrum at TRW. The specimen was mounted at the center for two of the axes. We can just rotate the box itself to get the two axes - with the plate impacted at the top. We mounted the box for the third axis on the bottom of the plate. The advantage of the plate over the shaker is that when you impact the plate at the end, you get a traveling wave shock; the transmission path approximates the real path much better; you do not over-correlate the input at the mounting points. The shock arrives at each mounting point at a different time, and the shock is closer to what it would be in real life. That is not quite the same for the third axis. You get these at about the same time, but the transmission path is attenuated by going through a couple of interfaces.

Figure 4 is the fixture, and it reflects the total set-up and the parameters that we had to work with. We had a compression system where we could put different compression loads into the plate to tune it to different resonant frequencies. The width of the plate and the length of the plate affected it. The weight of the hammer, the distance of the drop, all had an influence on the amount of shock we would get into the unit. We had some rubber compression members, and we have also tried several different materials to influence the shock response of the plate.

Figure 5 is a labeled picture of that system. Again, the unit was mounted at the center for two axes, and it was mounted at the bottom of the plate for the third axis. The slide hammer slides on the rod, it hits the anvil, and it transmits the shock down the plate and into the unit. Three curves (Figures 6 - 8) show the response spectrum and the tolerances that we got with the box mounted in each of the three axes. The data at the low frequency end are not really valid. However, we did a fair job of staying within the tolerances that were finally negotiated. Again, there is not too much difference in the data we got on each of the other two axes. You would expect the same from those two axes because the box is essentially merely reoriented. This particular fixture was developed by the Environmental Test department at TRW, and I thought, it did a good job by providing for our 4,000-g shock requirement.

We had other projects that had the same order of magnitude shock response requirement. Figure 9 shows a similar "Resonant Plate" system that did the same thing. This plate is a little bit different in length, and a bit different in width. The general arrangement and the technique are the same. Some of the things that we varied were the width and the thickness. We also tried aluminum plates and steel plates. You can vary a few parameters to accommodate some differences in your requirements, and we have had some success in this; this is the state of develoment of the resonant plate shock technique at TRW.

Figure 10 shows our approximate status at the present time. We have added a system to measure the force that we actually apply to the hammer, so we know what that force is. We use some Bungee cord, which is

not very elegant, but by adjusting the cord tension, you can get some added force to get a higher impact and vary the load and the acceleration that are input to the specimen. Again, it is the same general arrangement and technique of the resonant plate, impacting at the end, getting the transmission down through the plate into the specimen.

The next system is a "Resonant Plate" system that was developed by Lockheed. We are using it on a program that we are performing for them. The vertical impact in this case is perpendicular to the plate. The spectrum requirement we have is for one axis only. We get the response spectrum now in a single axis, and we don't have to meet a particular requirement in the other two axes, which simplifies the test requirement considerably. Tolerances are also more reasonable for this response spectrum. It is a 4,200-g peak response spectrum, and we mount the specimen in two different orientations so that we do get some variability in the amount going into the component.

Figure 11 shows a sketch of the general test arrangement. The specimen would be mounted on the plate, and a pneumatic actuator impacts the plate. It is an aluminum plate about 1/2 inch thick and its size is 4 feet by 6 feet. The plate has a 3-inch foam pad underneath; the rest of the structure and control panel are built up to support the plate and handle it.

We have a single axis response spectrum that we are trying to meet with this particular arrangement: a 4,200-g proto-qual requirement. Figure 11A shows the response spectrum that we will be obtaining with that system. Figure 12 shows that test set-up. Again, the unit is mounted on the plate, and you can vary the distance from the impact point to the test unit. Some damping material can be put under the hammer, and in this particular case, it is some paper and a felt pad. The pneumatic actuator is controlled by a panel. Figure 12 shows the foam and the plate. Figures 13 and 14 show the test setup from the opposite end. You can see the foam pad and the plate a little better in Figure 13. Figure 14 is essentially the same as Figure 13; you can see the hammer and the damping material.

Figure 15 gives a pretty good idea of how well Lockheed did in meeting the requirements with this particular piece of equipment. The 4,200-g spectrum with the tolerances is shown, and they met it pretty well except at the low frequency. Then we tried to vary some of the parameters. The distance was 2 1/2 inches from the unit to the measurement point, and the distance from the measurement point to the impact point was 6 1/2 inches. The actuator pressure was 150 psi and 15 sheets of paper were used for damping; that is not real elegant, but it does the job.

We decided we wanted to know what would happen if we changed the actuator pressure, and Figure 16 shows that. We have gone to 250 psi, using the same damping and the same distances. The higher pressure has raised the whole spectrum. Then we thought we would try changing the damping. We got 26 pieces of paper; we use the same pressure, 250 psi, and the same distance; the damping knocks off the tail of the high frequency-response (Figure 17). It did a very nice job; it shows what you can do by adding a little damping if you want to bring the high frequency end down. Then we tried varying the distance to the impact point. We went from 6 1/2-inches to 8 1/2-inches. It knocks down the whole high frequency end of the spectrum, not just the tail of it, not just one end. So increasing the distance between impact point and unit brought the entire high frequency range down nicely (Figure 18).

Figure 19 shows the effect of adding a felt pad which as you can imagine, changed the damping considerably. That is evidenced by the amount of tail-off we got at the high frequency with much more damping. So, the few things that you can vary on that system don't look like they are very significant but you can do quite a bit with the spectrum by just varying a few of the parameters.

Initially when this requirement was imposed, we didn't have a very good idea of what kind of shock this would put into our components. We took exception to the requirement until we could get some feel for the response on a specified plate, since Lockheed did not have the data at the time. We collaborated with Lockheed on a test. We supplied an instrumented unit, just a dummy mock-up of a couple of slices of electronics that are typical of the type of equipment that we will be using on this project (Figure 20). We had many response accelerometers mounted inside the test unit for this test. Figure 21 is a prototype of the system that we are using on this project. It is a little bit different but essentially the same set-up. We took data at the input to the box, and we measured some responses inside to see how much attenuation we were getting.

I mentioned we are doing this in two axes of orientation. Figure 22 shows the other axis where we are mounted face-on to the shock wave as opposed to the shock wave coming in from the unit edge. Figure 23 is the same picture but with the labels on it showing the impact hammer and the pneumatic cylinder forcing the hammer down against some damping pads on the large resonant plate. When you hit the plate the plate goes into some sort of resonance. To get what we wanted on this particular test, the plate was free at the middle, and we had foam at each end of the plate. So there are several ways that mounting the plate can be handled.

Figure 24 shows the instrumented test unit. We mounted accelerometers at the top to find out how much of attenuation we got all the way up. We mounted some accelerometers right near the mounting feet to find out what we were getting across the mounting interface and we mounted many accelerometers inside the unit to show how much acceleration we got, inside in the middle of the boards and where parts would be located, because we have sensitive parts that we are concerned about. Our major concern was whether the parts inside the components could survive the 4,000-g shock requirements imposed on this resonant plate when we were not exactly sure what type of attenuation or amplification would occur.

Figure 25 shows the instrumentation we had inside. An accelerometer is inside at the middle of some mocked-up boards to get responses inside. Figure 26 shows close-ups of the accelerometers at their mounting points.

Figure 27 shows the instrumentation mounted at the corner. If you want to generalize on the attenuation that we got going from the input of the box to the inside the box, it is about a 3 dB attenuation. It is not as much as Hank Luhrs measured in some of his spacecraft simulator tests, or what we really expect to see on a real spacecraft. On a real spacecraft it is probably 6 dB or more. If you look at the way vibration specifications are developed over limit load, there is about a 2-1 margin on that. So the 2-1 margin on the shock is probably not too bad a margin, as long as you recognize you have a margin when you are testing, and you are overtesting the equipment. The margin is over and above what you would see in real life. The test is not too bad considering you want a margin for qualification. If you design to meet this type of requirement, you should be in good shape in real life. That is the purpose of the qual test.

Discussion

Mr. Mardis (General Dynamics - Pomona Division): I had seen this apparatus before. How much did it cost? How did you establish your material selection and the contact geometry between the hammer and the plate?

Mr. Morse: I don't have an exact answer on the cost. You can see from the material we used to put it together, it is not expensive. However, quite a few dollars were involved in the development work to arrive at the system that was shown. We did quite a bit of work on several programs with it, so the cost to TRW, to develop the three particular plates that we showed, probably does not represent what somebody like you might have to do to go into a program now, because you have a pretty good idea of where to start. With regard to material selection, we initially tried steel plates, and they ring much more than aluminum. Probably, if you use magnesium you can get more damping. So, you would have to look at your particular requirements and try to tailor the materials that you want to use toward the spectrum that you have and the levels that you have from the other parameters. "The details are left to the student." About the contact geometry, each of those hammers that you saw in the figures shown are slightly curved so it is not a pointed impact point, but it is rounded in a fairly small area. In many cases we did use a Delrin washer at the impact point. We tried different thicknesses, and different thicknesses gave us different levels. So you would probably end up doing some development work to develop your particular spectrum with the impact point and using very different materials. We used a steel hammer and an aluminum anvil.

Mr. Rosenbaum (General Dynamics - Convair): I guess we at General Dynamics should talk to each other more because we have been using an impact tester for seven or eight years that we made out of an old HYGE machine which we use for the pneumatic hammer. We have done a similar type of testing for electronic components for a long time.

Mr. Morse: Many years ago I was very irritated at the methods used for high impact shock, and I thought, "Boy, the Navy is really unscientific with their high-impact medium weight shock machine. They just have a hammer hitting a plate." I thought, "How could anybody be so unscientific as to just hit a plate with a hammer and expect to get the right shocks." Yet, the Navy stuck with that system, and the equipment that went into submarines and ships passed those tests, and never had problems. In retrospect, it was not a bad system. They didn't have much data on it. They may not have known exactly why they were doing it, but they were doing it right; maybe for the wrong reasons, but probably for a lot of the right reasons. So you have to temper some of the judgments you make as a young fellow as you get older.

Mr. Rosenbaum: In retrospect, talking about what Henry Luhrs was talking about this morning, we found "off-the-shelf" 250-g relays that would barely pass drop-table or drop-tower shocks but they would easily pass orders of magnitude more than that on a resonant-type table like this.

Mr. Morse: I discussed the advantages of the resonant plate in a paper that I presented three years ago. One of the advantages of the resonant plate is that the transmission path is simulated and the shock arrives at the box in a similar way as it does in the real world. Also, you do not overcorrelate the input as you do on a drop table or on a shaker. These are the major aspects of simulation that the resonant plate performs. The mounting impedance is still not good because you are using a plate instead of a honeycomb panel, a spacecraft structure, or whatever the real structure is. Although the compliance still is not matched, you at least match the transmission path in some respects.

Mr. Rosenbaum: Like you, we have used felt, rubber, Delrin, and all kinds of things by trial and error to vary the widths of the pulse.

Mr. Morse: Again, this just shows a couple of ways to solve the shock simulation problem. You have a problem. You want to perform a shock test. How do you do it? "Resonant plate" techniques are not too bad. You have a few things that you can vary; you can vary some of the parameters and get where you want by using paper for damping or whatever works.

Mr. Dotson (Lockheed): I was involved in the development of this system. One reason we tried this approach was we were trying to develop a system that would cover a particular spacecraft that had many items mounted on honeycomb panels, and which had very similar characteristics to a flat plate. It was also a system that had a lot of low frequency response. We started off trying to use explosive joints to excite this plate. It was hung vertically at the time, and we found we couldn't generate the low frequencies. In checking one of the accelerometers one of the technicians happened to hit the plate with the sledge hammer, and we got the exact spectrum we wanted. All of a sudden we got real excited about it. So, we laid it horizontally, and because we didn't have an air-impact device at that time, we just dropped an aluminum cylinder down a plastic tube; lo and behold, we were getting all kinds of good results. Then we started varying the parameters; the thickness of the plate, the type of material, the damping material, and the distance from the source. We put it on foam, we put it on sand, and we could vary the spectrum shape widely. It was very successful. I should mention that by putting it on sand, you can move the low frequency modes and steepen the slope. So that is another parameter if you ever need it. As an aside, something that came out of this is that this is a

single directional-type device: but many companies who wish to use this device require three-dimensional equality. We have recently put book-end shelves on the system, and we are getting significant in-plane responses. It is still vertical, but you can rotate the box on the shelf. Another point is when data are taken on a spacecraft, or even on this plate, many times you have a tri-axis accelerometer that is mounted on a little aluminum block or some other type of device. The rocking of the plate, or the rocking of the structure in the spacecraft, can give you what you think is a longitudinal or an in-plane response that is not really there. It is not true in-plane motion. So beware if you are mounting your accelerometers above the neutral axis of the structure or plate if that is the case, because up to about 7,000 Hz and higher, just the rocking effect is equal to the normal response. Once we went back and looked at all of our spacecraft data, we realized that all the enveloping of what looked like in-plane response was really a rocking effect due to normal response due to the bending waves. So, I think many of the three-dimensional spectral requirements came from non-true three-dimensional effects.

Mr. Morse: That is a good point. I have to agree with what is mentioned in the lead-off chart: that our requirements are only for a single axis. In a previous project we had to meet the same shock requirements in all three axes. If you look at the way components are mounted in a spacecraft, shock really comes to it along a single axis. You don't get the same response in all three axes. You get a major response perpendicular to the plane on which the component is mounted. You don't get the same response in the other in-plane axes. I think that it is well recognized in vibration because many vibration specifications now require different levels in-plane, and normal to the mounting plane. I think that eventually the method should be the same for shock requirements.

Voice: The last two or three minutes of discussion satisfied the comment I was going to make. It was about the compromise that one might have to accept in a lateral axes, but I thought Ron addressed it real well by saying with book shelf-type fittings you can get the lateral axes as well.

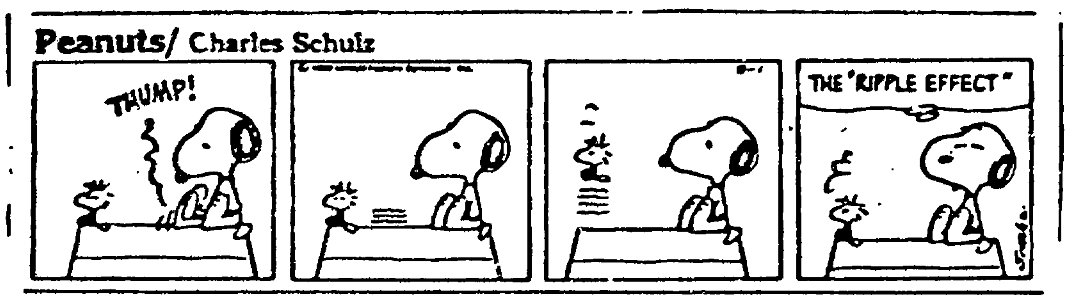

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Figure 1. Concept and Use of Resonant Plate System

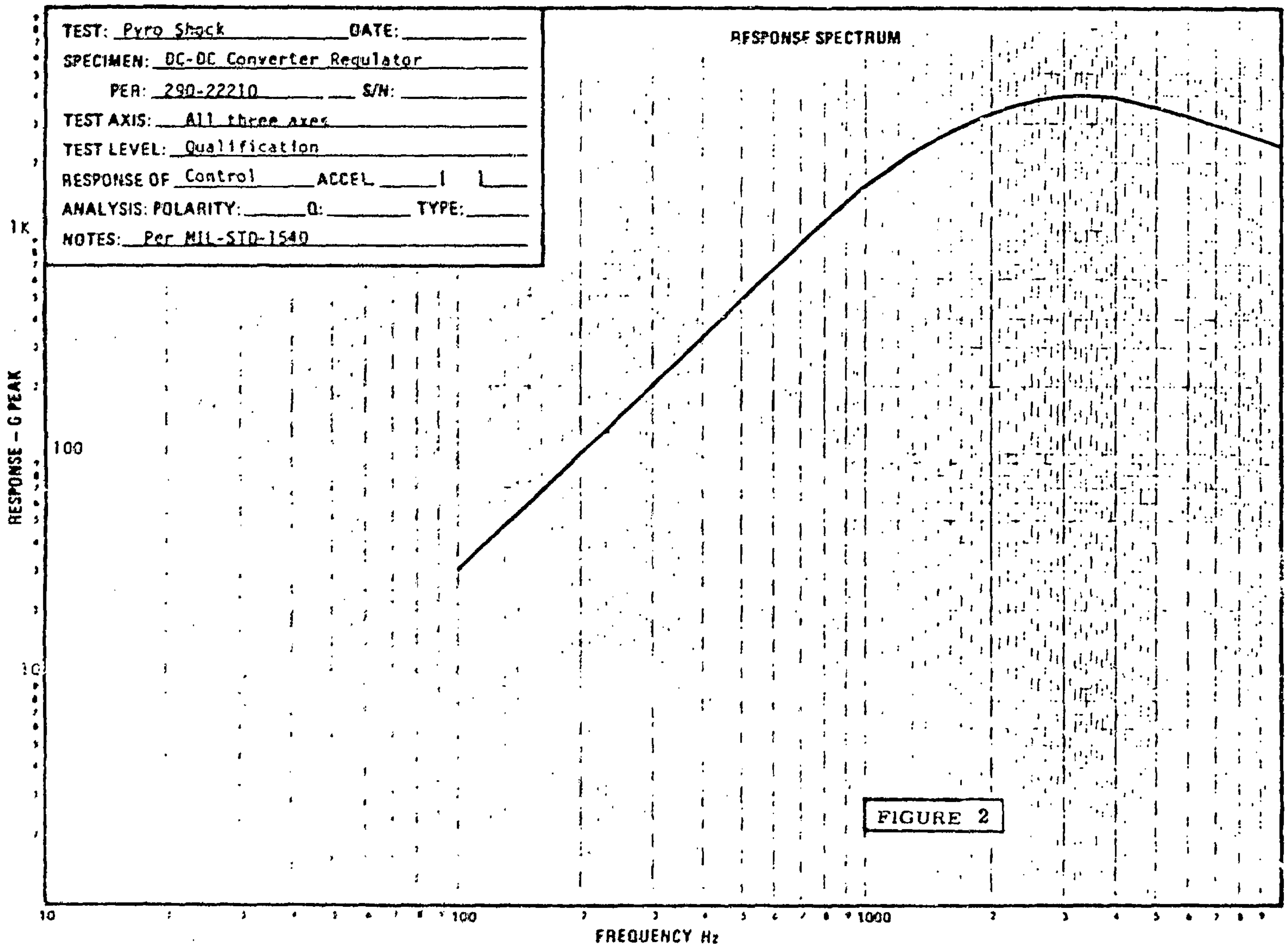


Figure 2. Component Response Spectrum

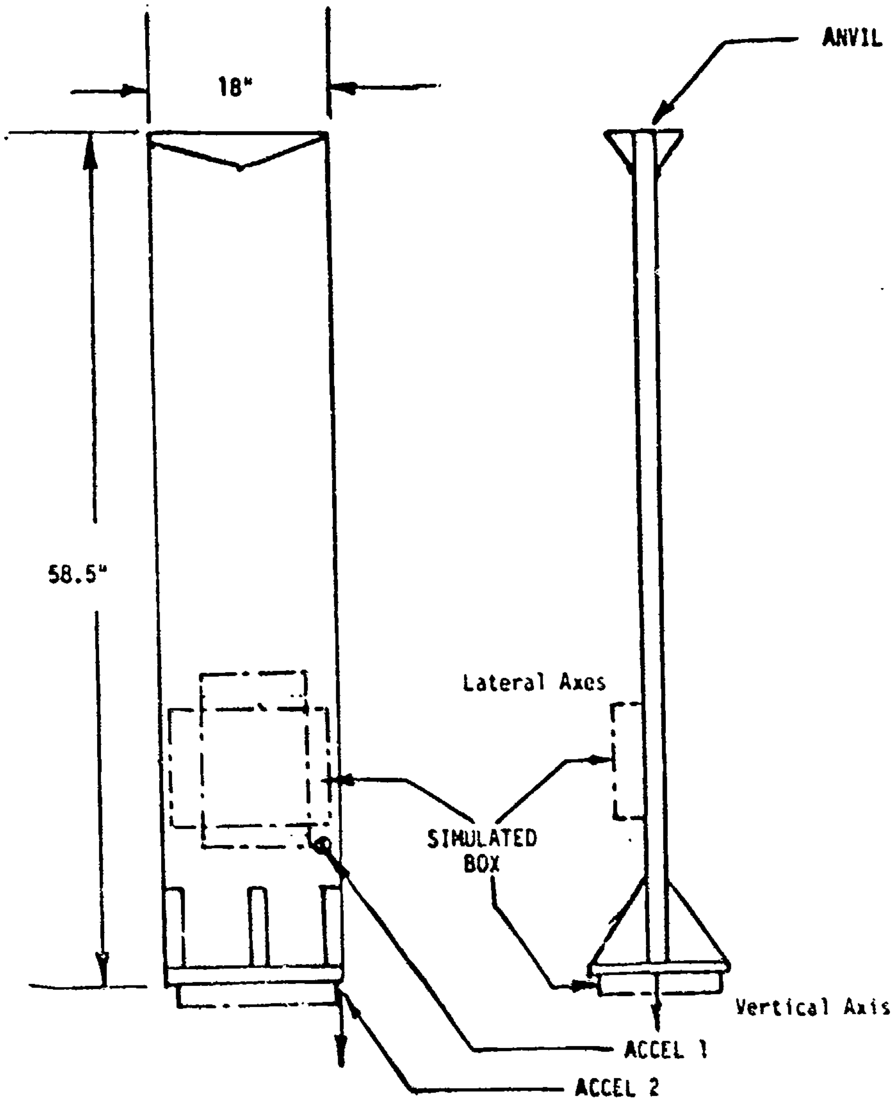


Figure 3. Resonant Plate Developed to Meet a Component Response Spectrum

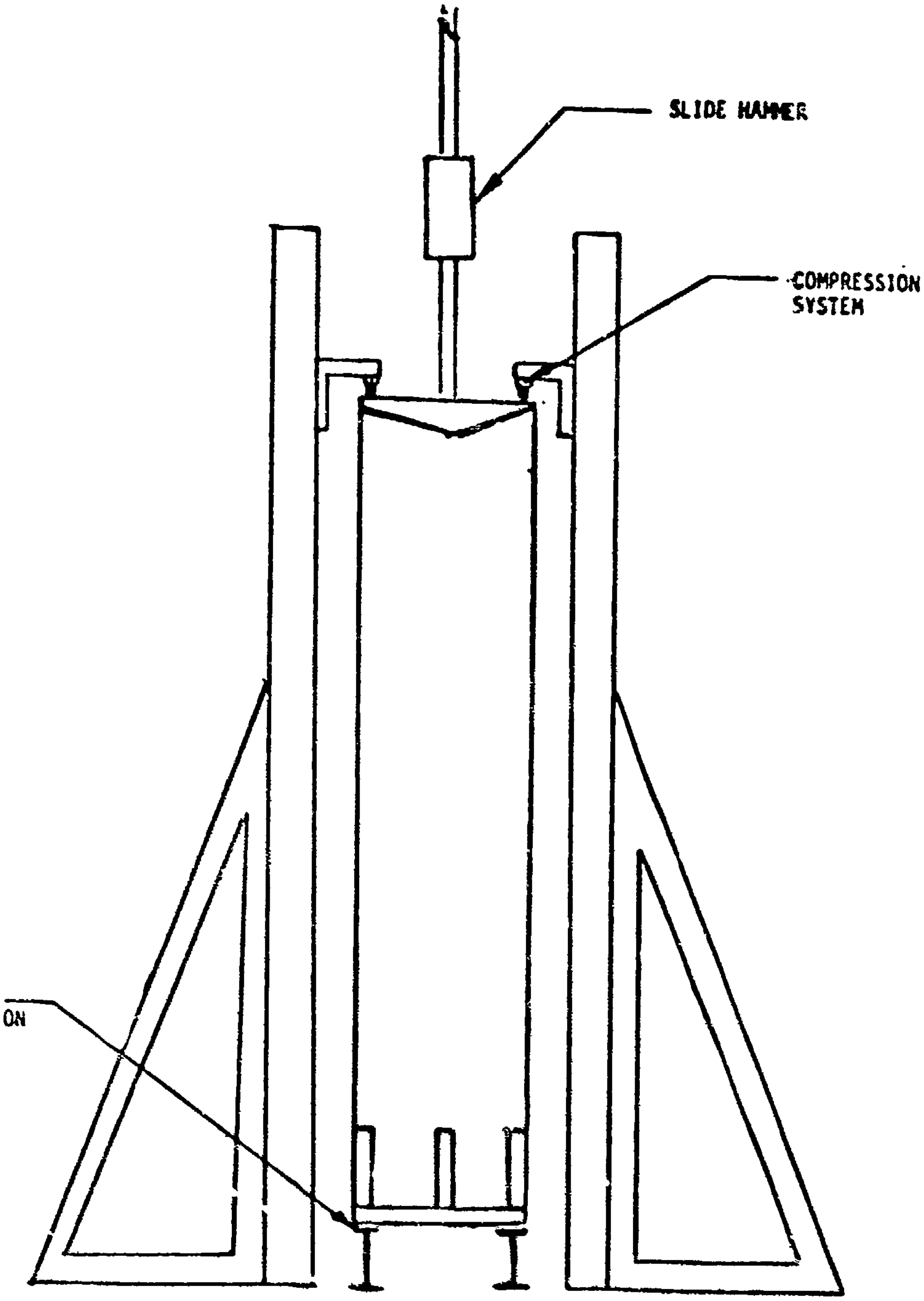


Figure 4. Test Fixture and Test Arrangement

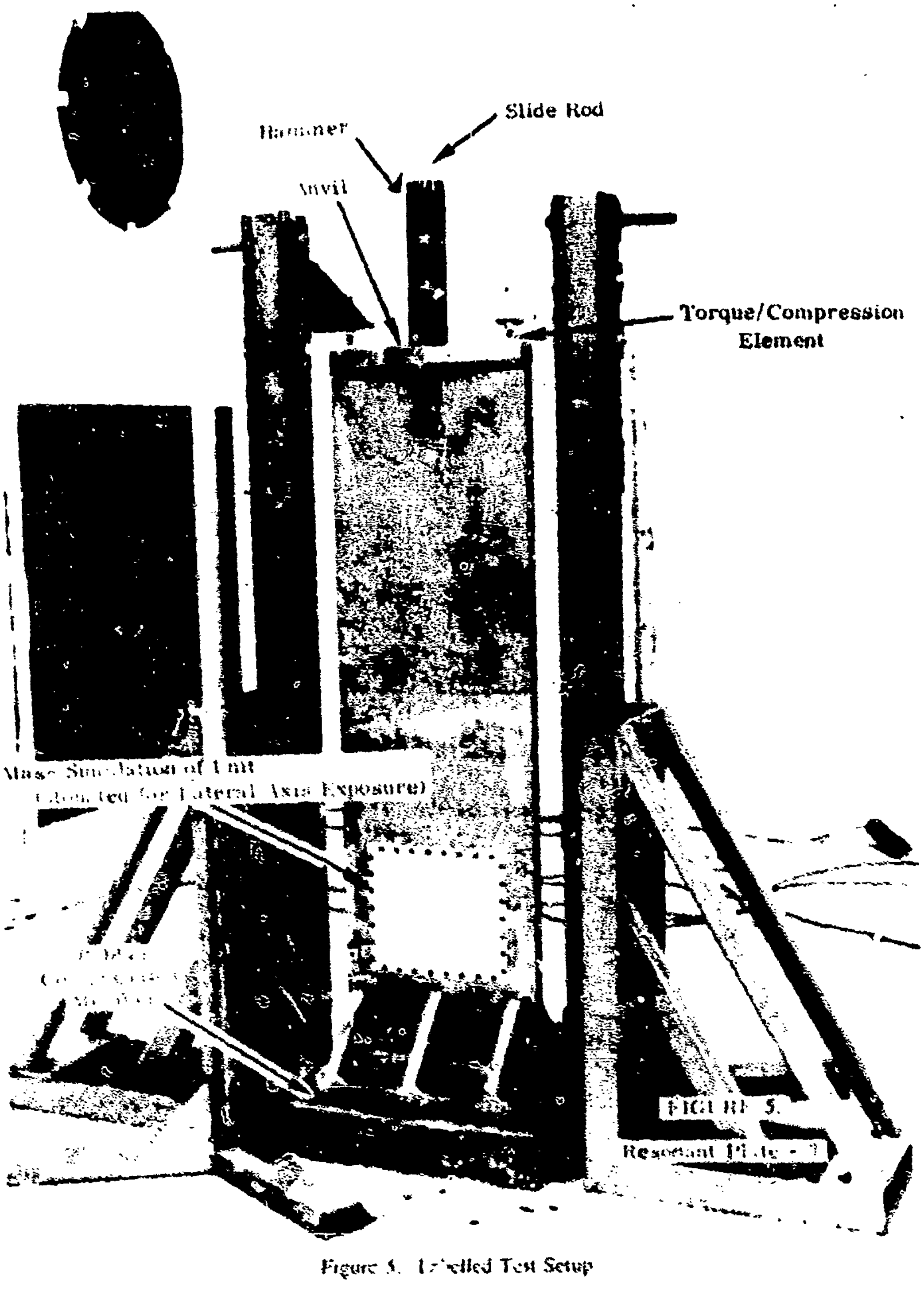
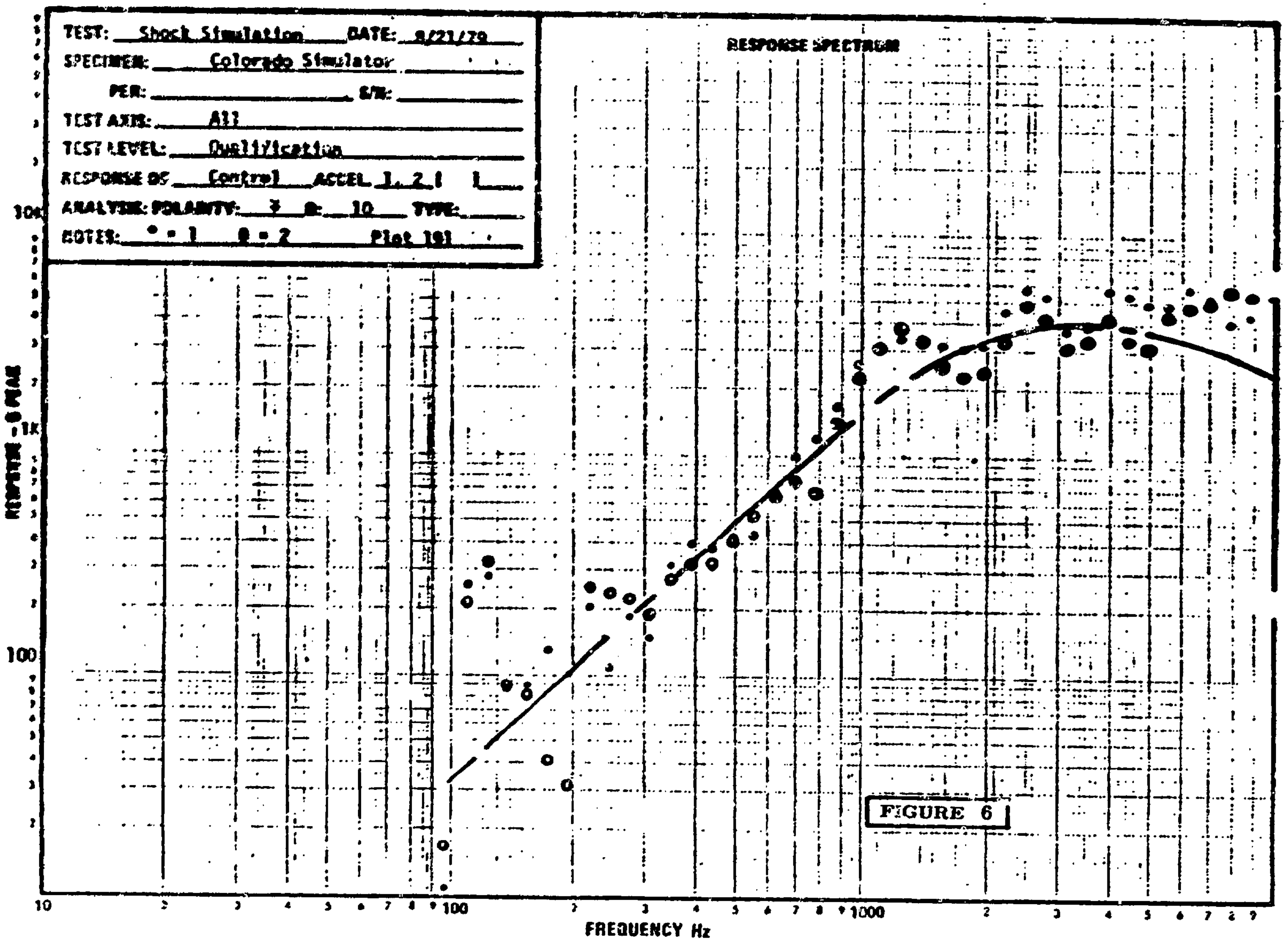
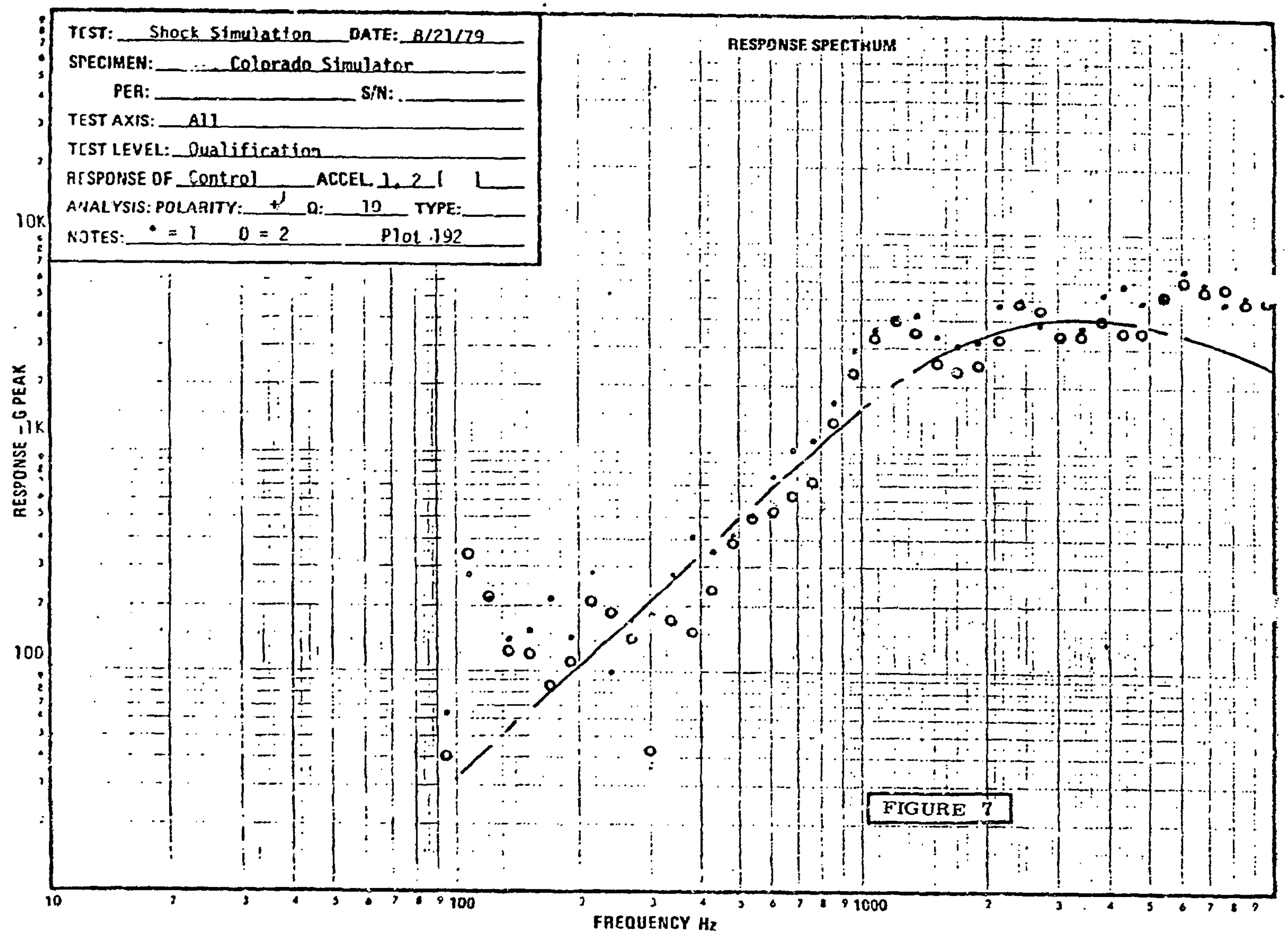


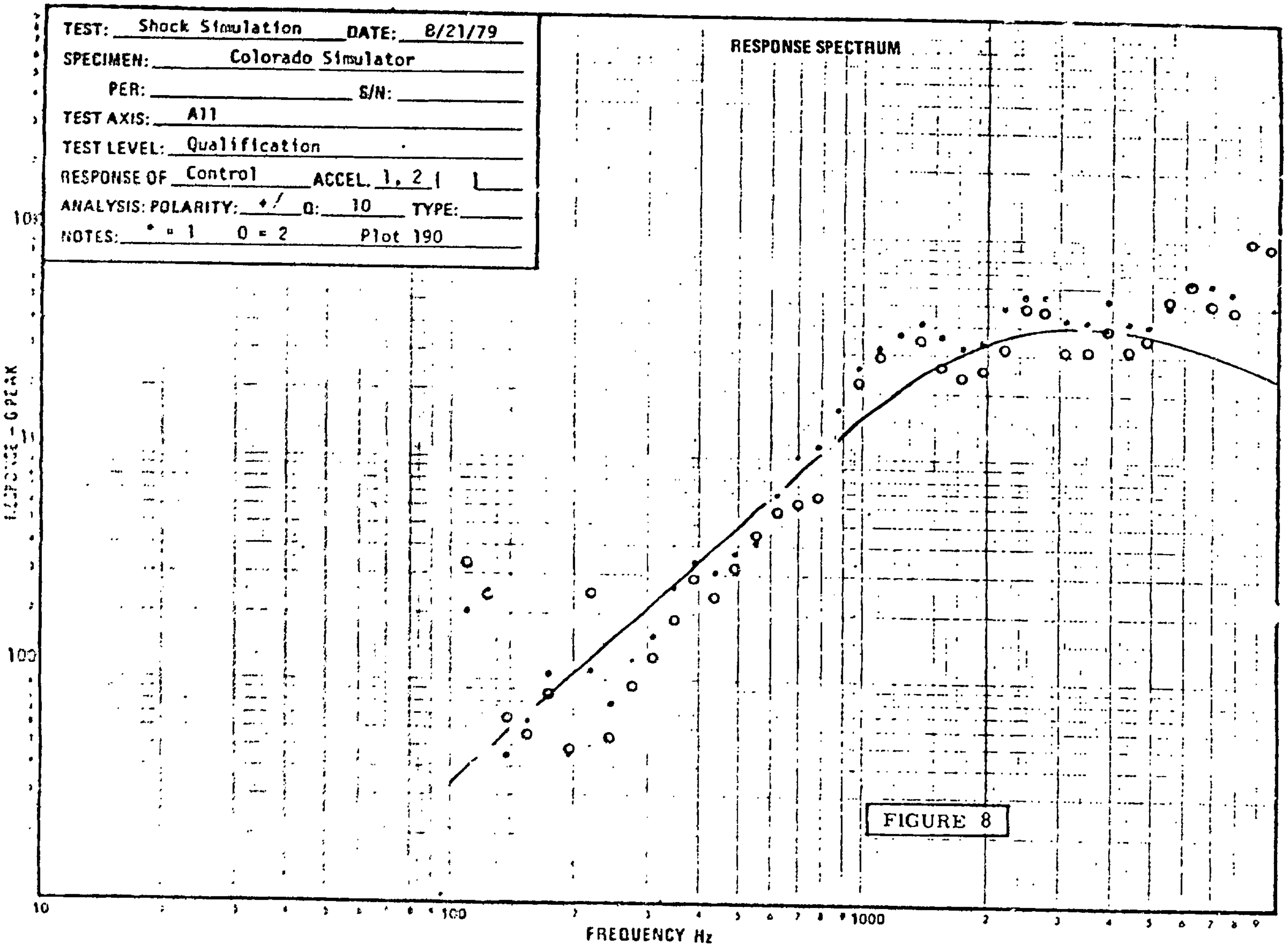
Figure 5. Labelled Test Setup



Figures 6 Results of Simulated Pyro-Shock Tests



Figures 7. Results of Simulated Pyro-Shock Tests



Figures 8. Results of Simulated Pyro-Shock Tests

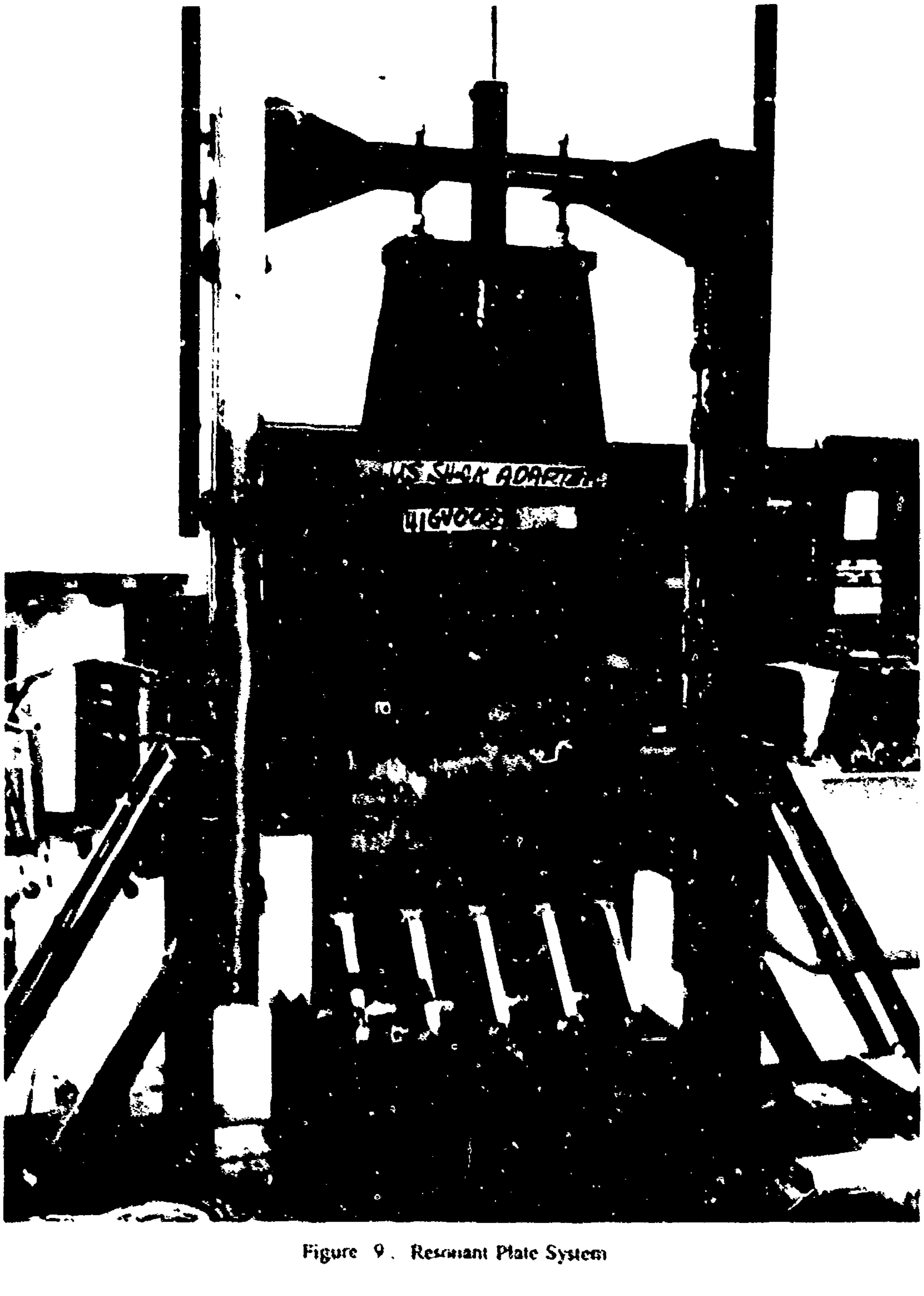


Figure 9. Resonant Plate System

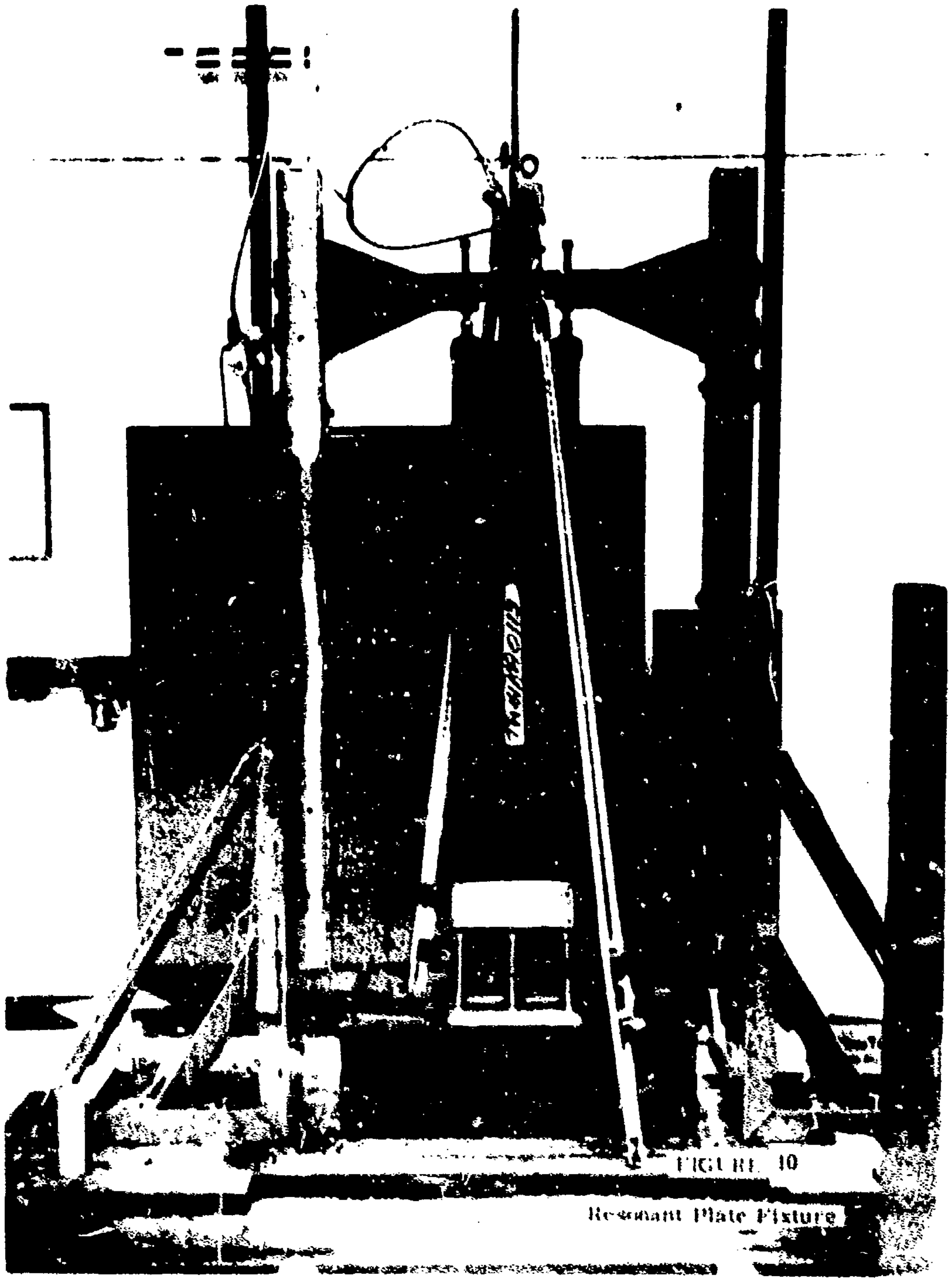


Figure 10. Resonant Plate System

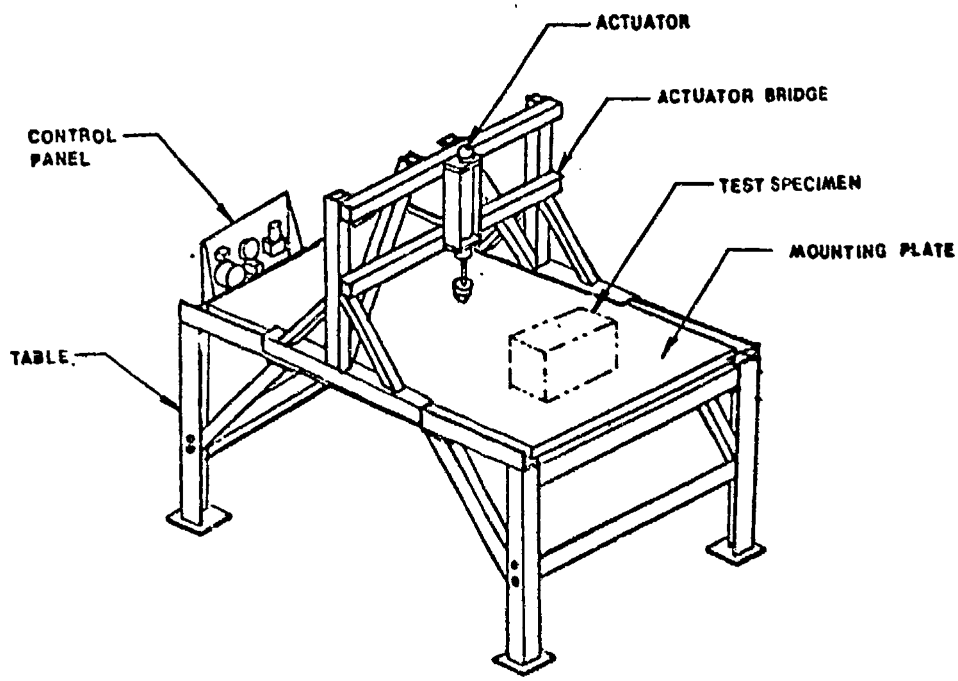


Figure 11. General Test Arrangement Using the Lockheed Resonant Plate

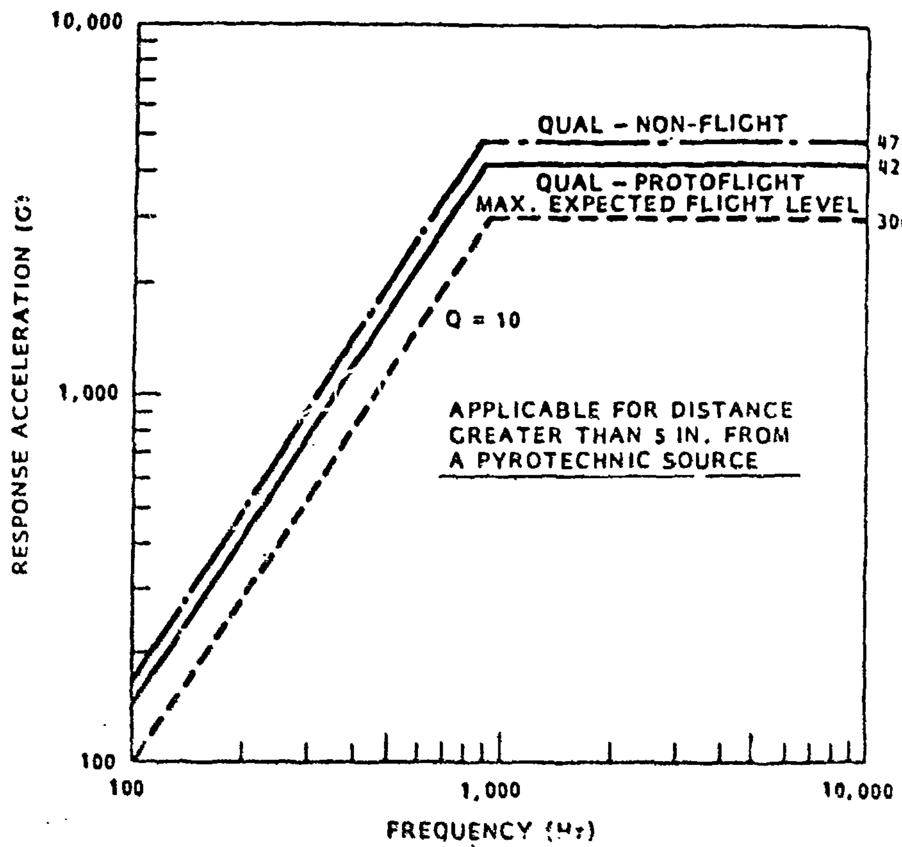


FIGURE 11A PYROTECHNIC SHOCK ENVIRONMENT

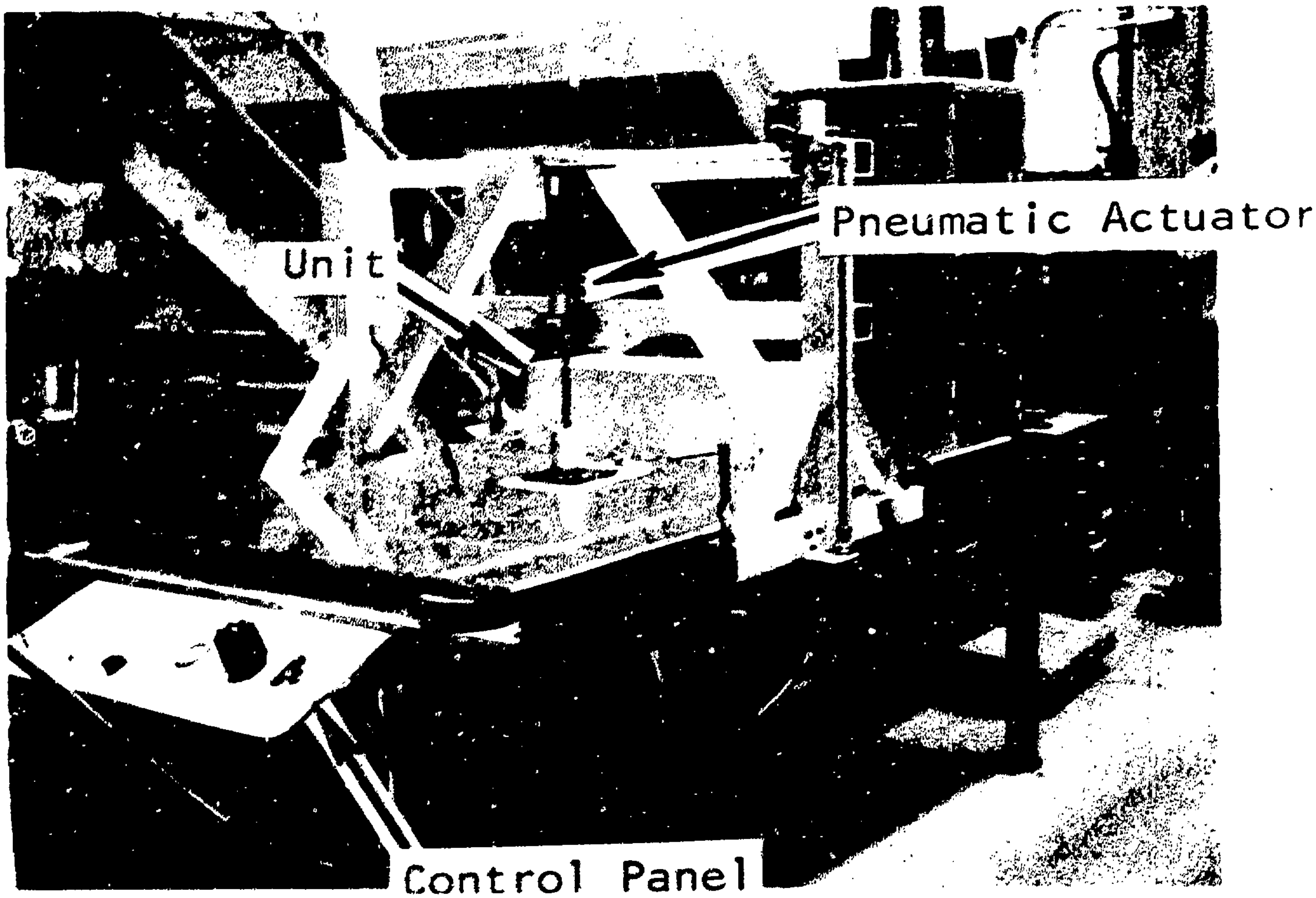


Figure 12. Proto-Qual Test Setup on Lockheed Resonant Plate System

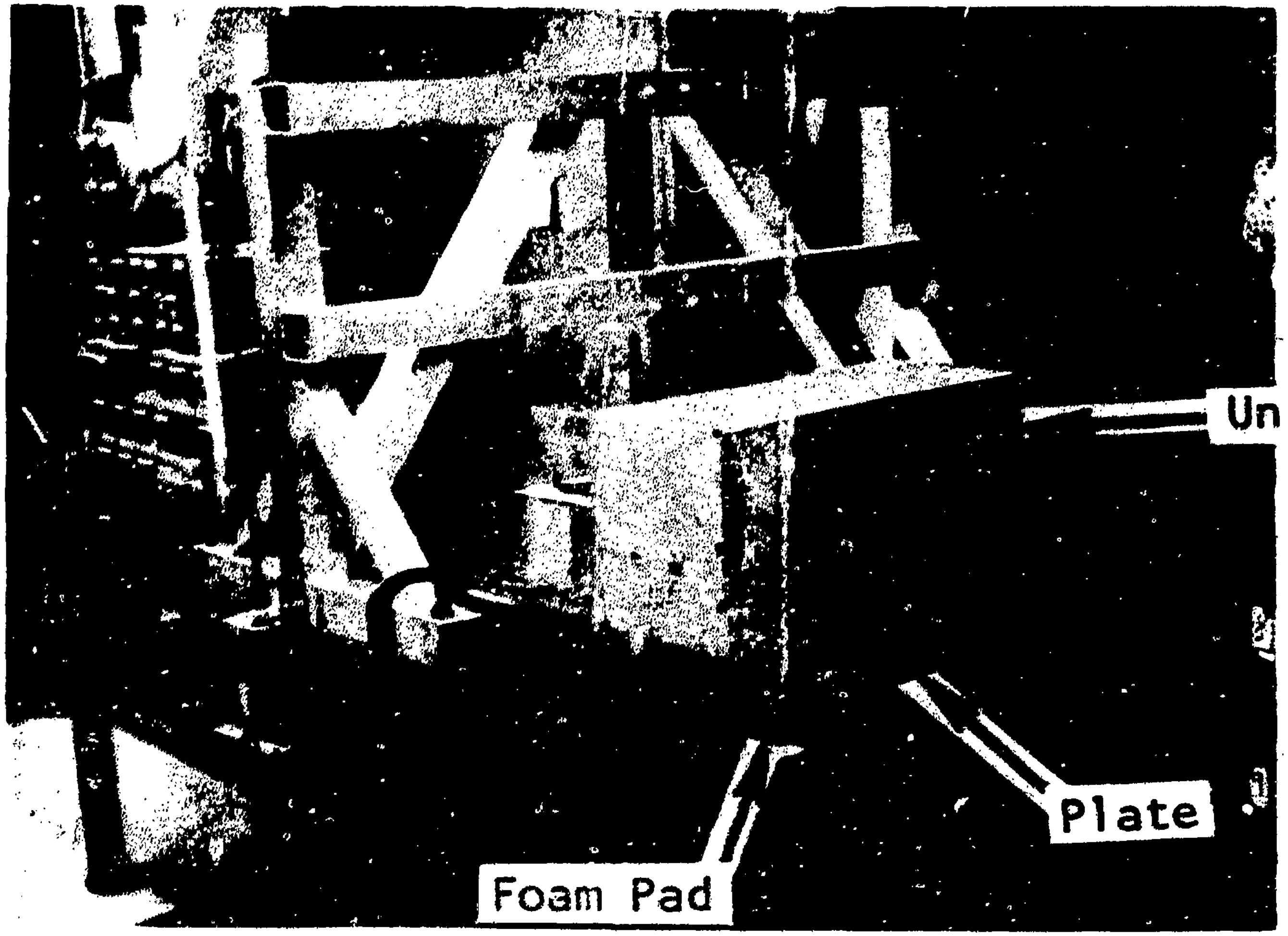


Figure 13 Other Views of the Proto-Qual Test Setup on the Lockheed Resonant Plate

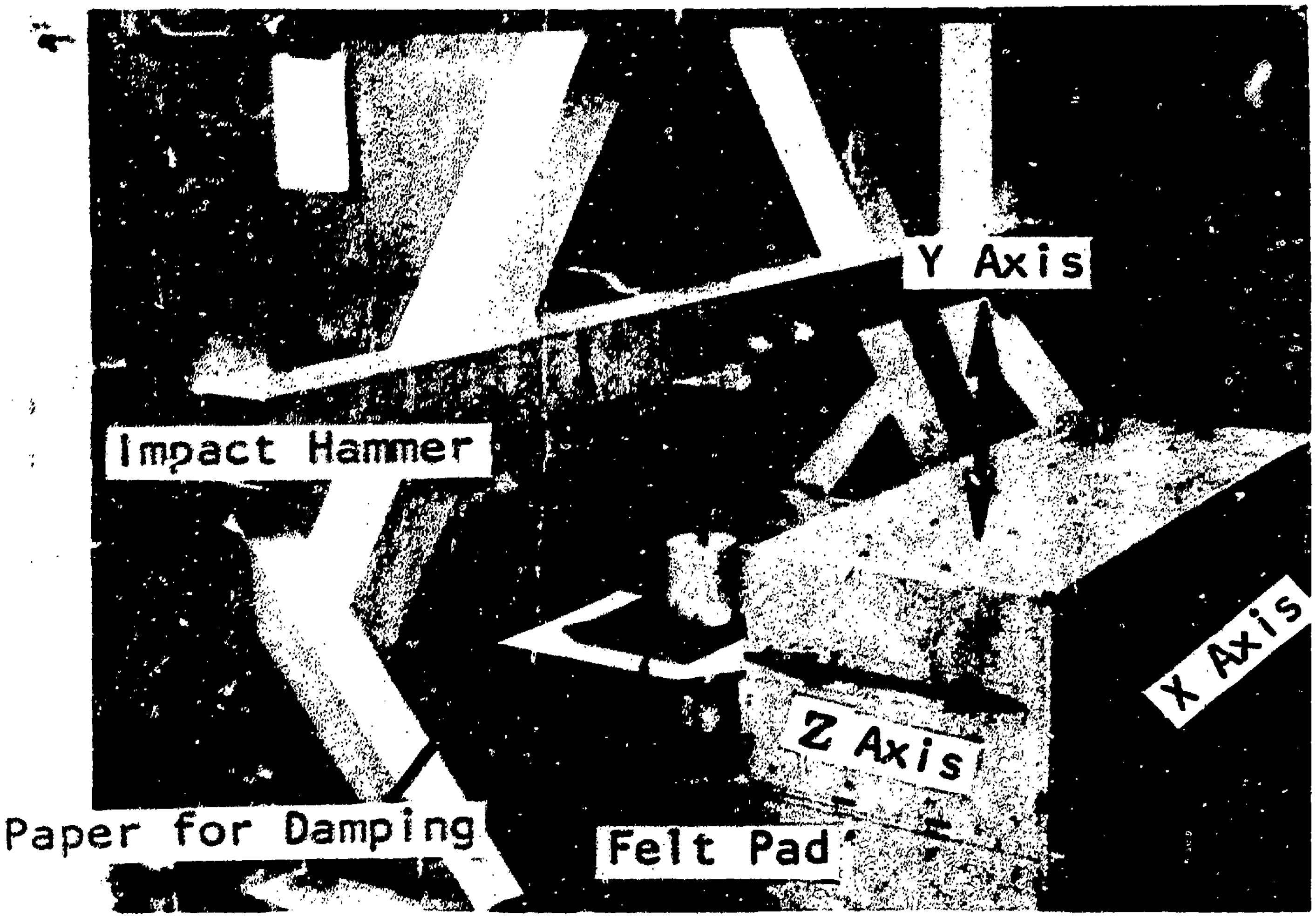
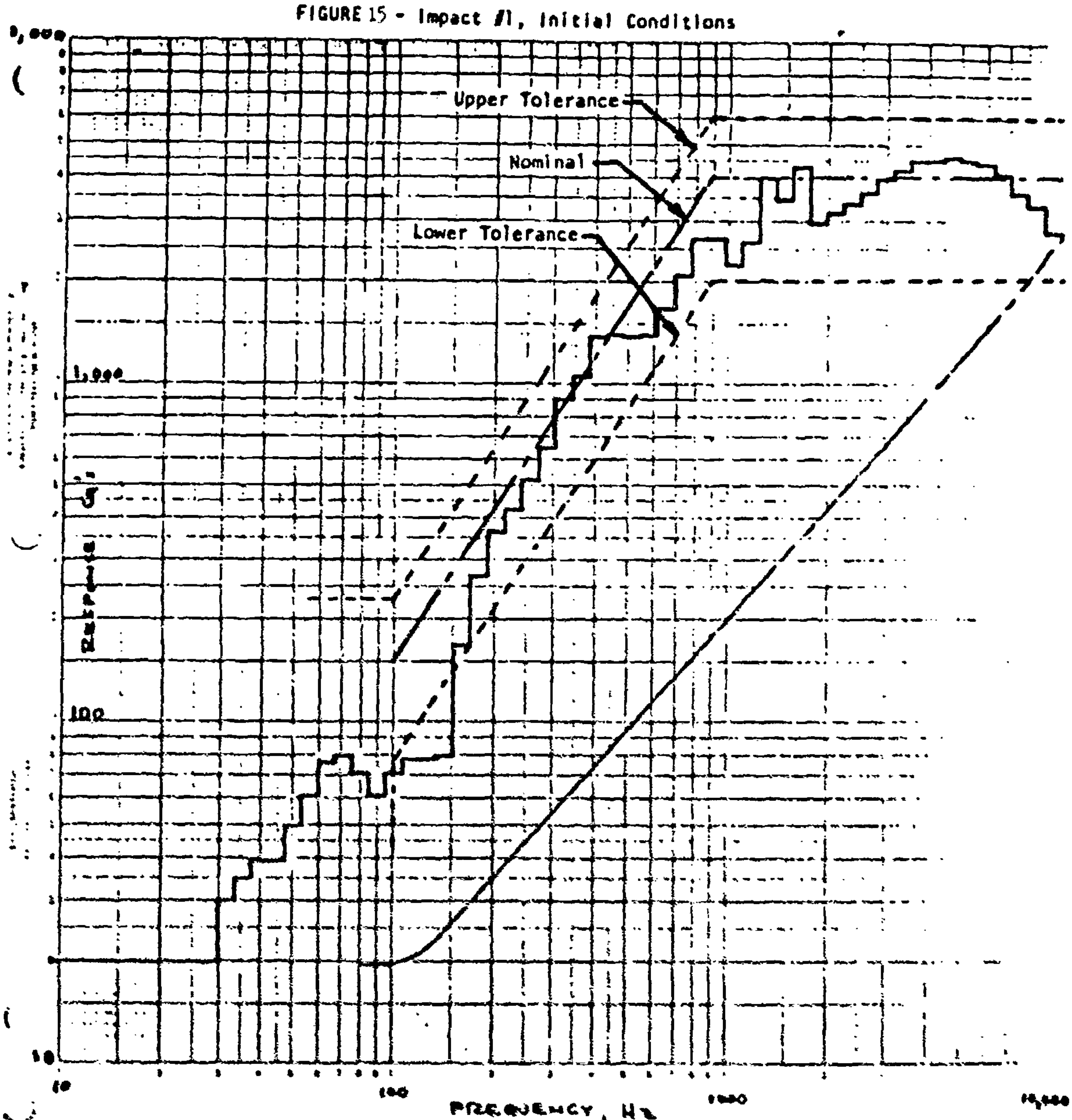


Figure 14. Other Views of the Proto-Qual Test Setup on the Lockheed Resonant Plate



Distance - Unit to Measurement	=	2.5 inches
Distance - Measurement to Impact Point	=	6.5 inches
Actuator Pressure	=	150 psi
Damping	=	15 sheets paper

Figure 15. Response Spectra Measured on Lockheed Resonant Plate System

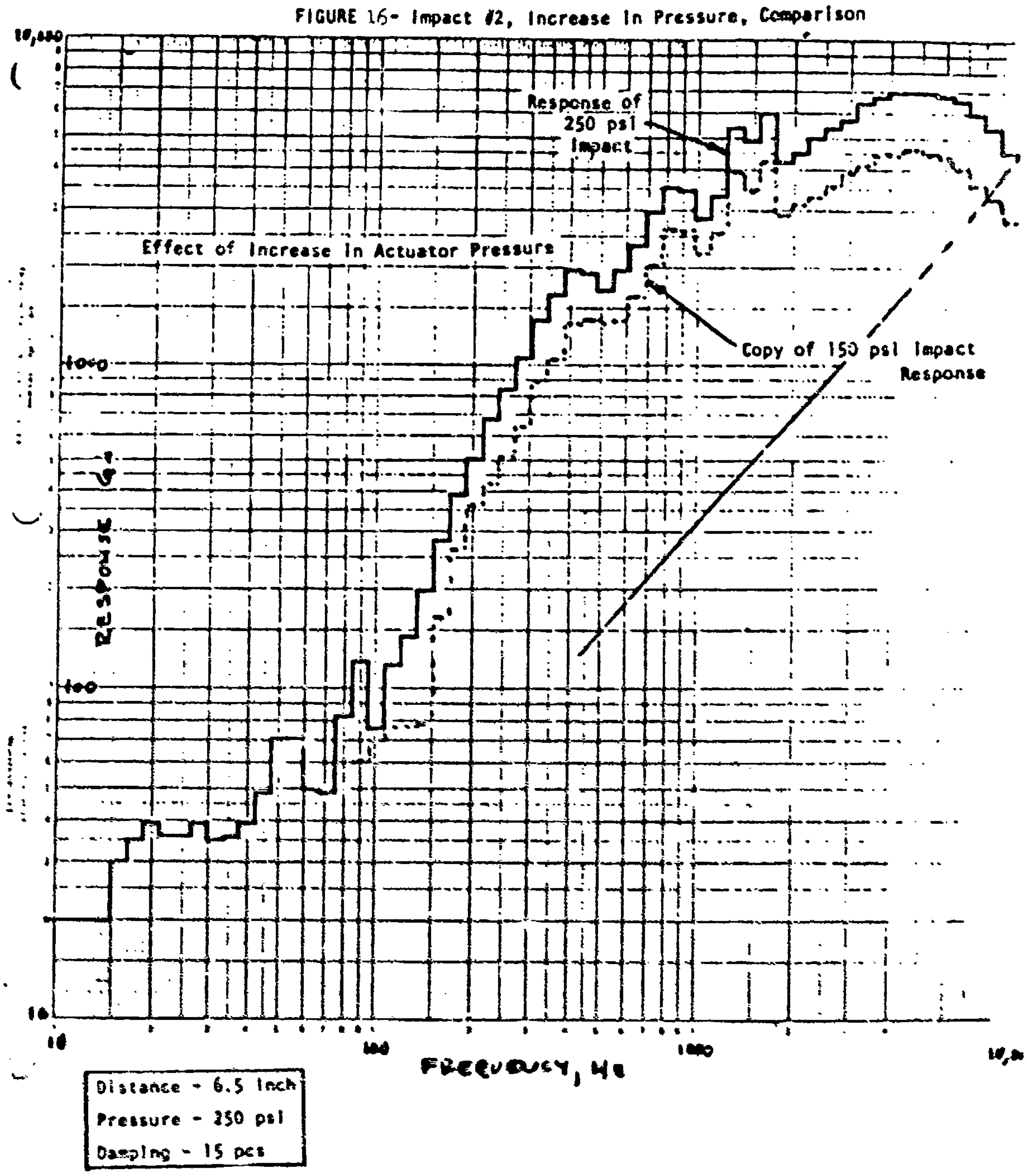


Figure 16. Effect of Change in Actuator Pressure on Response of Lockheed Resonant Plate System

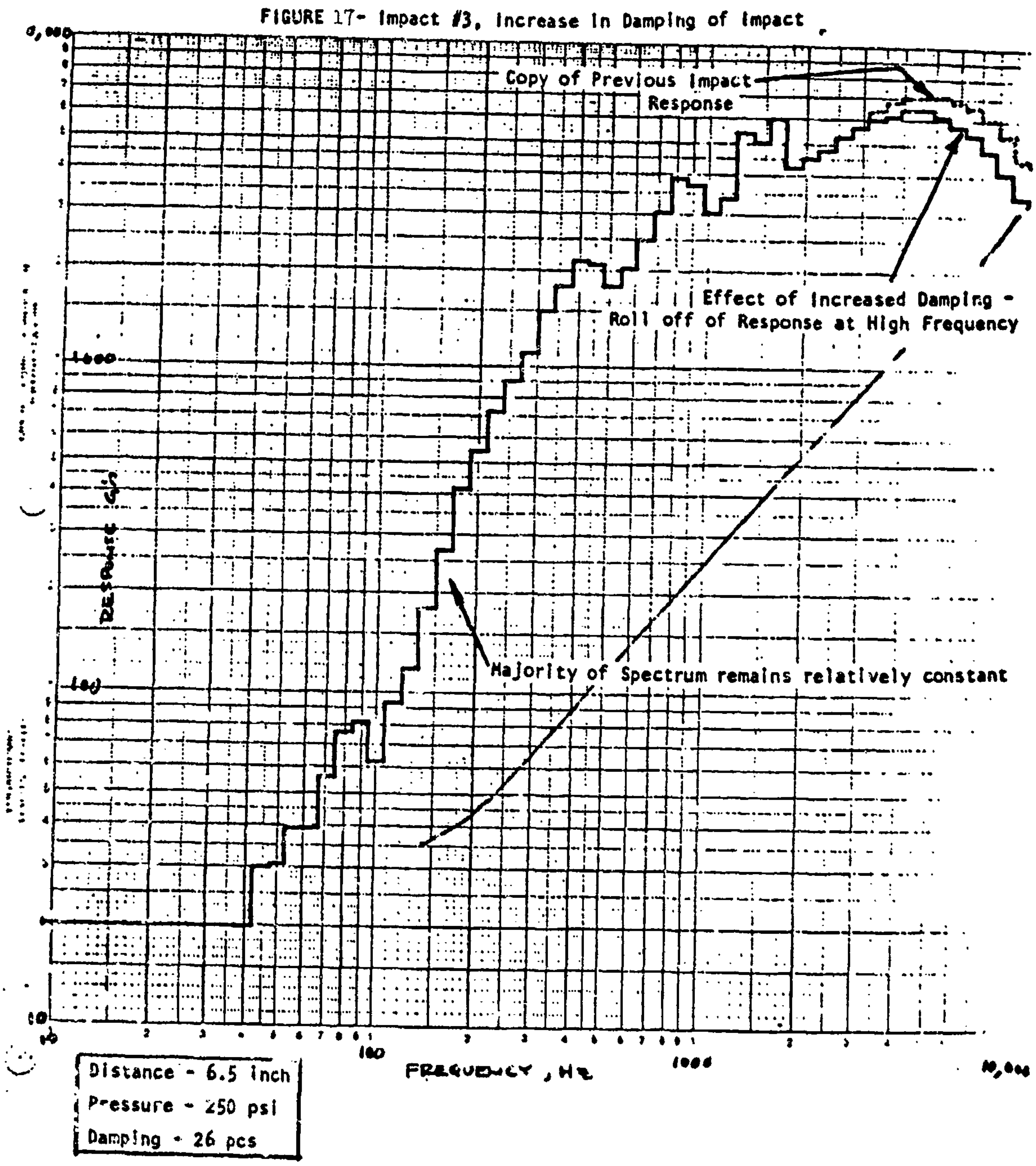


Figure 17. Effect of Change in Damping on Response of Lockheed Resonant Plate System

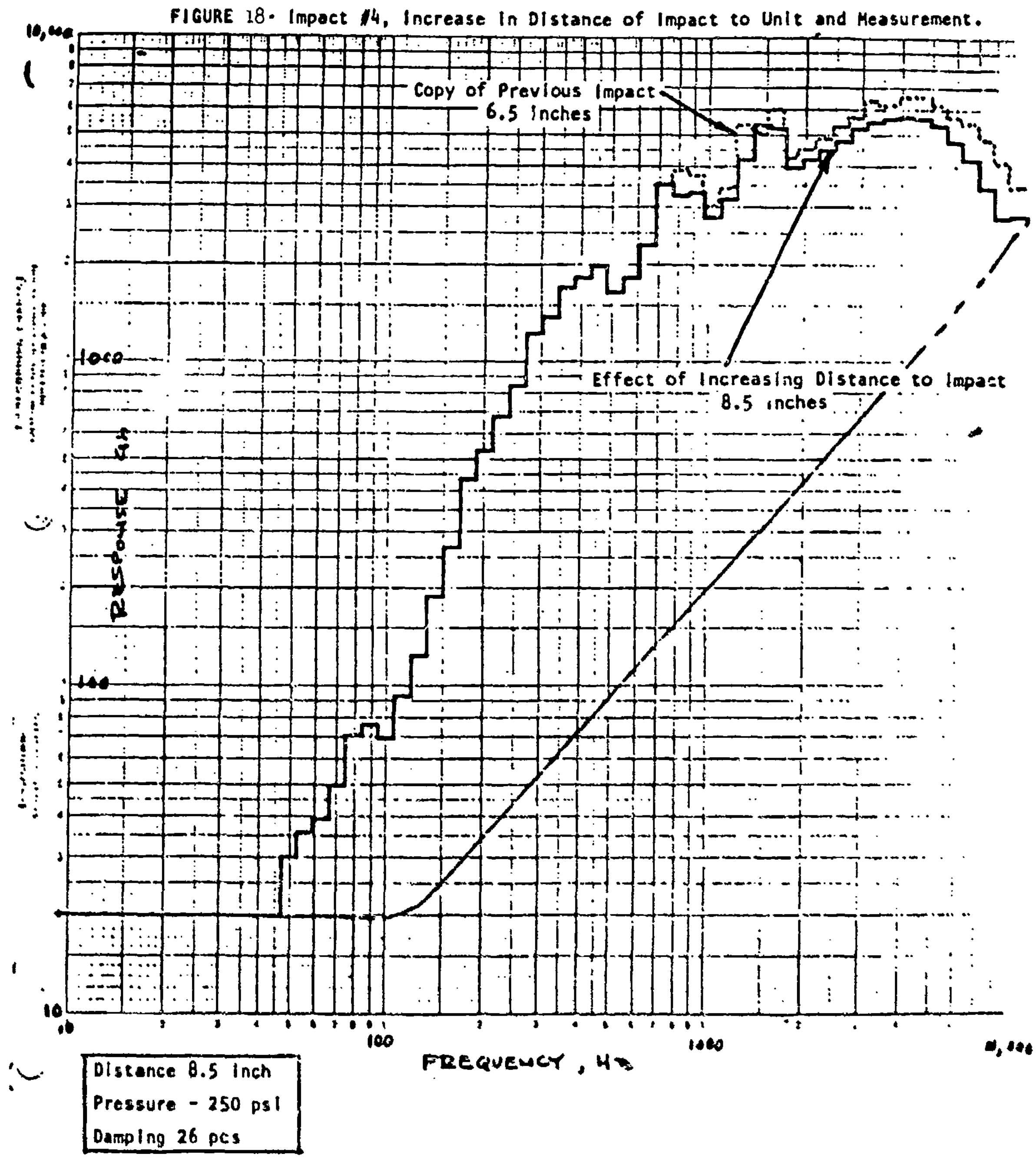
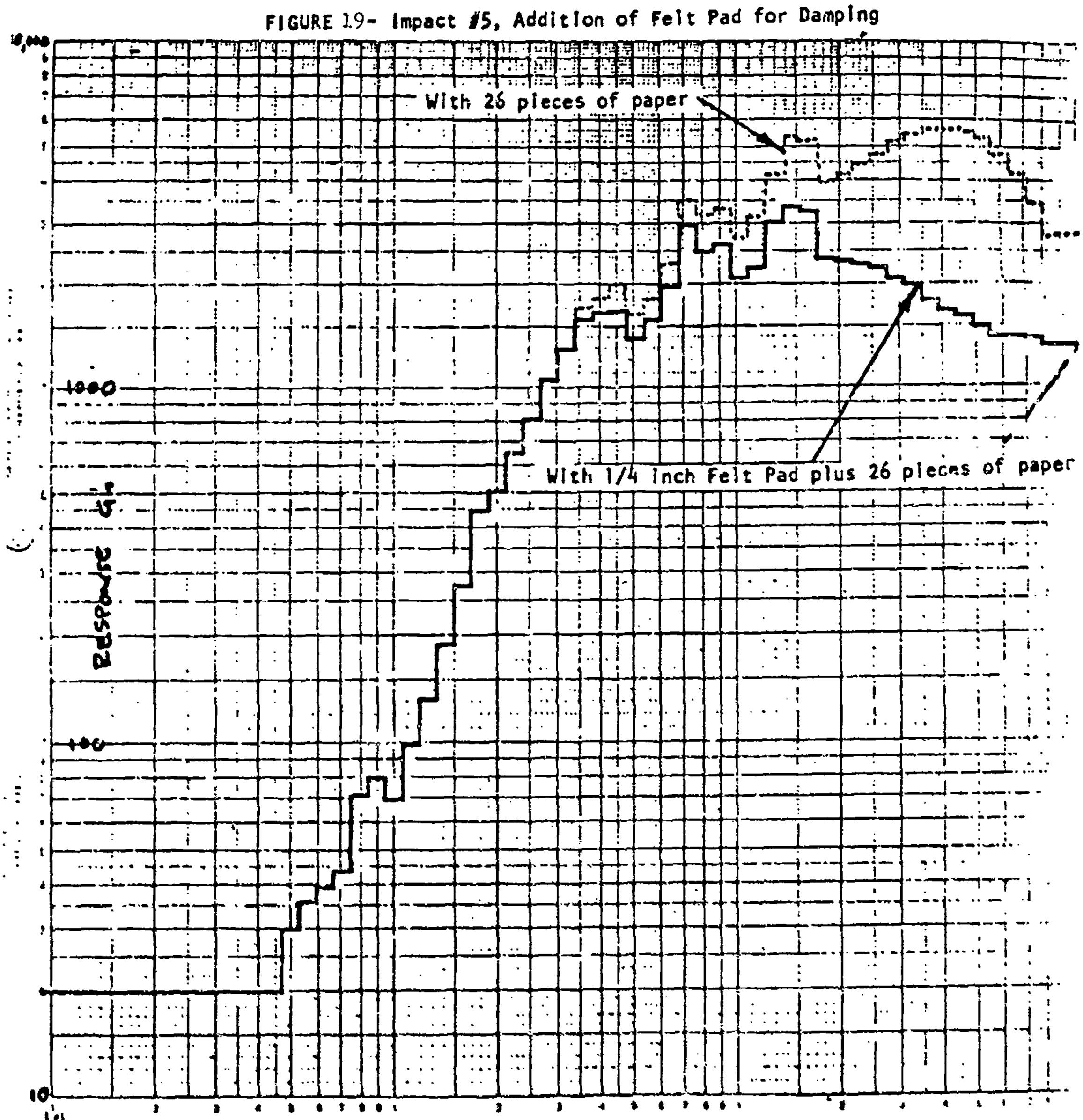


Figure 18 Effect of Change in Distance from Impact Point on Response of Lockheed Resonant Plate System

FIGURE 19- Impact #5, Addition of Felt Pad for Damping



Distance - 8.5 inch
Pressure - 250 psi
Damping - 26 pcs + .25 in Felt Pad

Figure 19 Effect of Addition of Felt Pad on Response of Lockheed Resonant Plate System

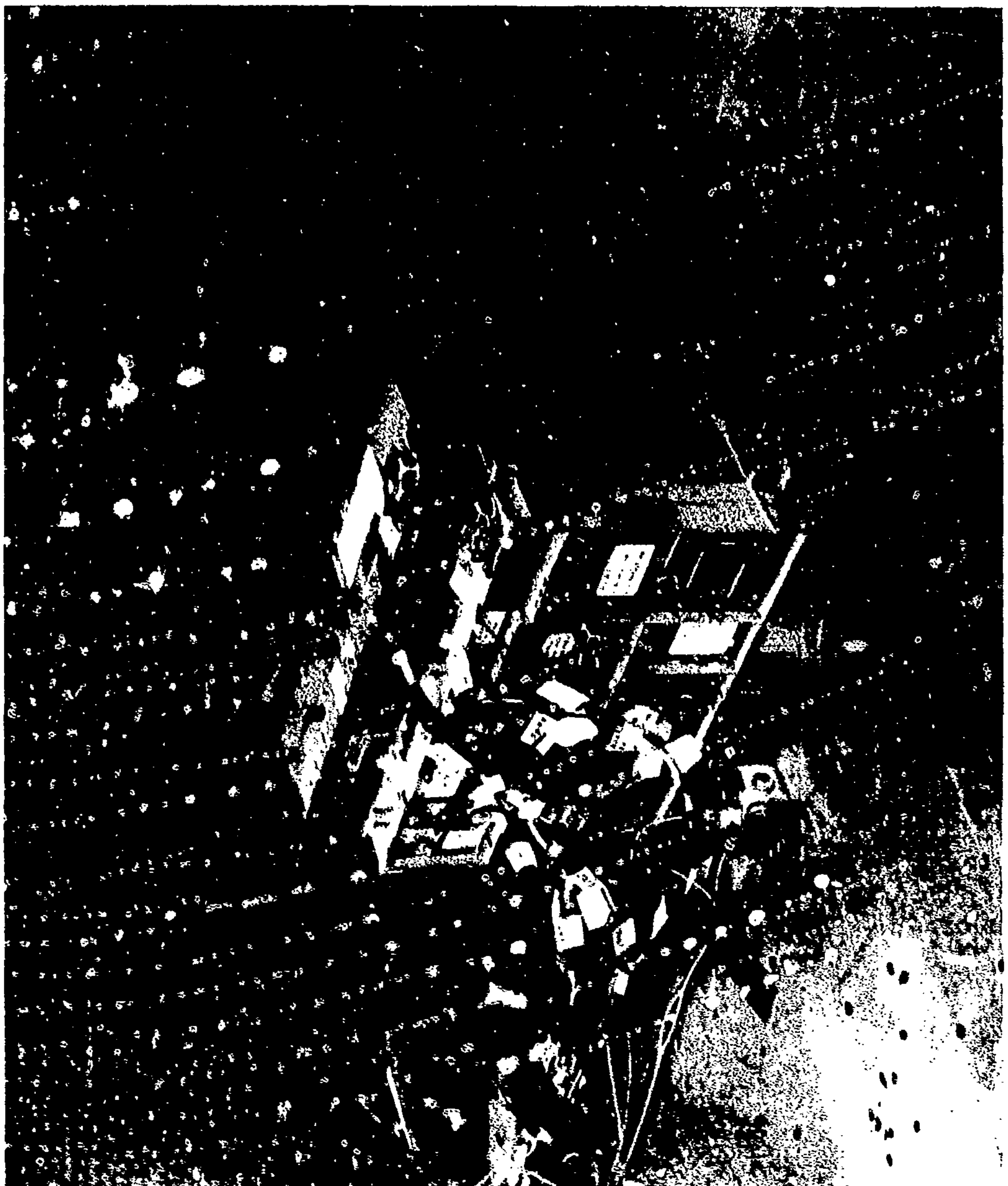


Figure 20. Mockup of Electronic Equipment

Figure 21. Prototype Electronic Equipment Test Setup
Side-on to the Wave Transmission

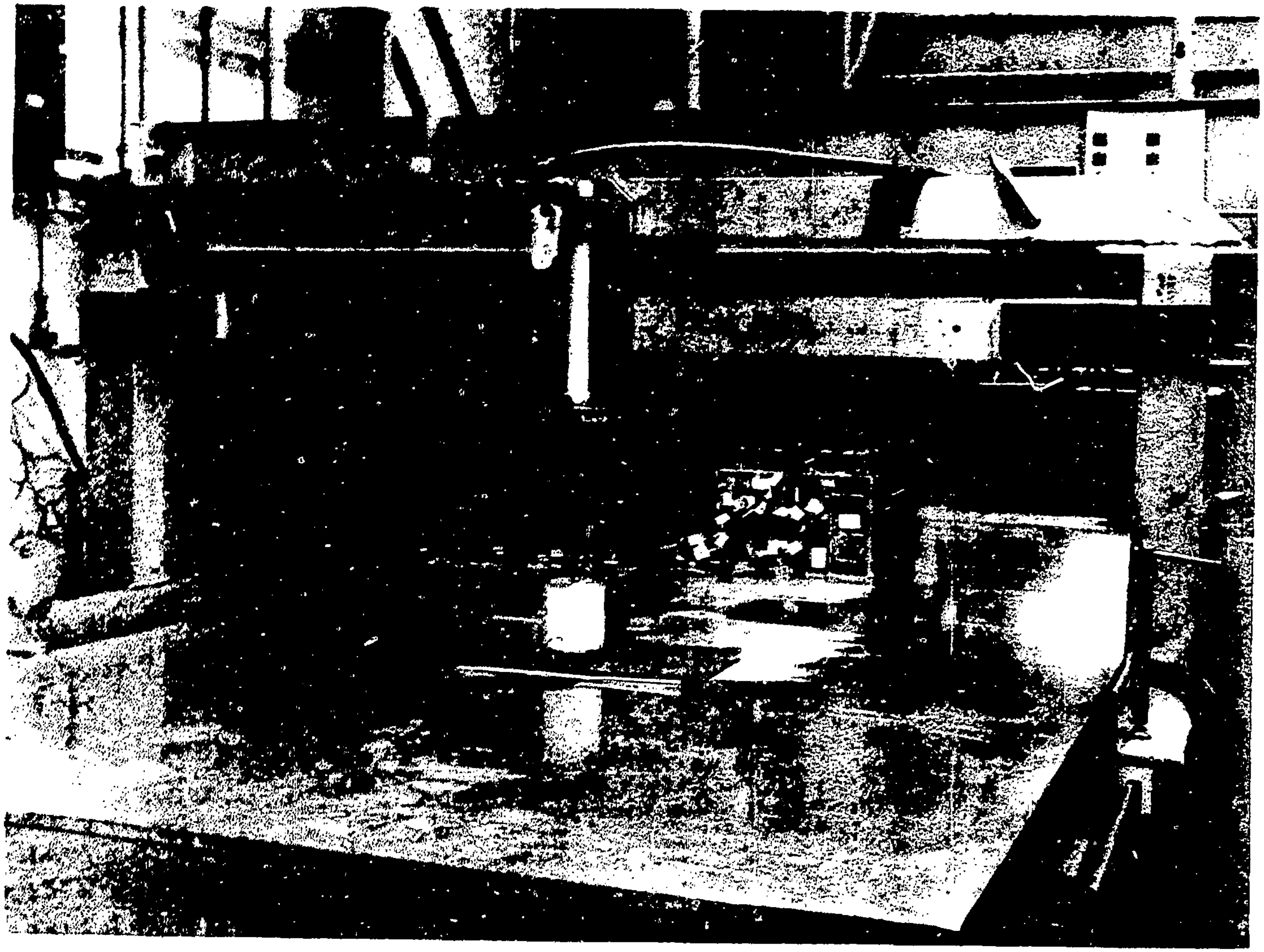


Figure 22. Prototype Electronic Equipment Test Setup Face-on to the Wave Transmission

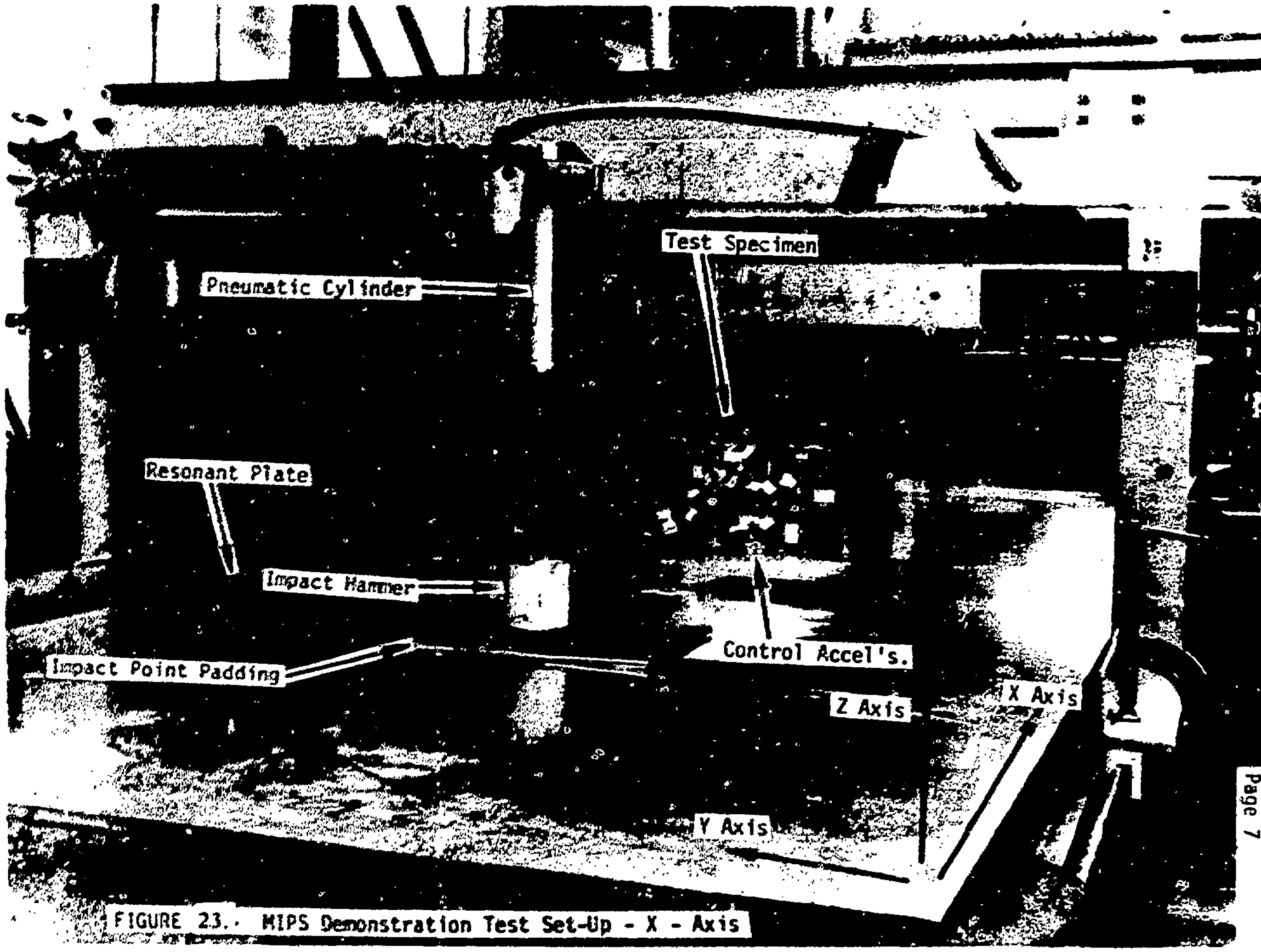


Figure 23. Prototype Electronic Equipment Labeled Test Setup
Face-on to the Wave Transmission

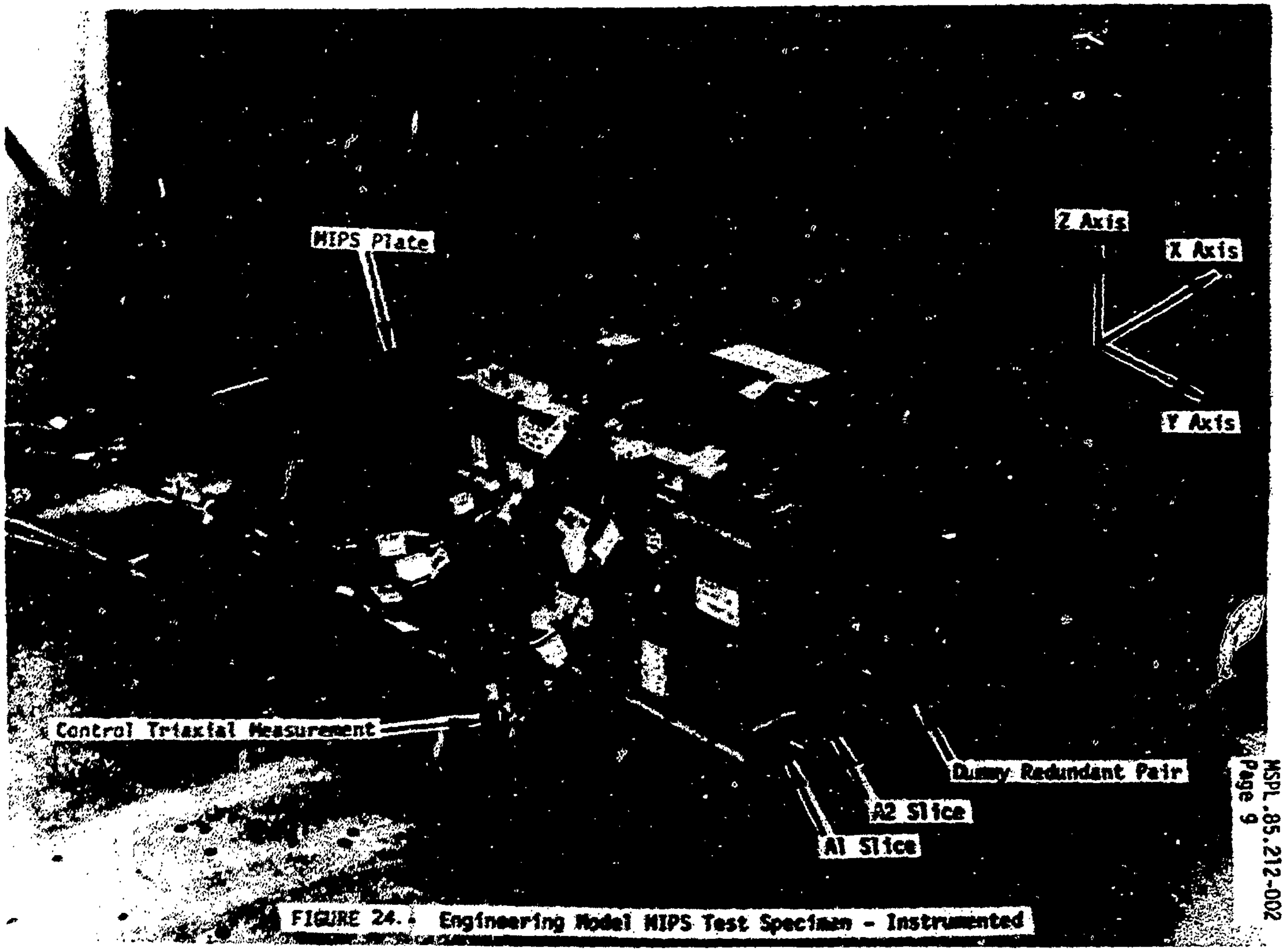


Figure 24. Instrumented Test Unit

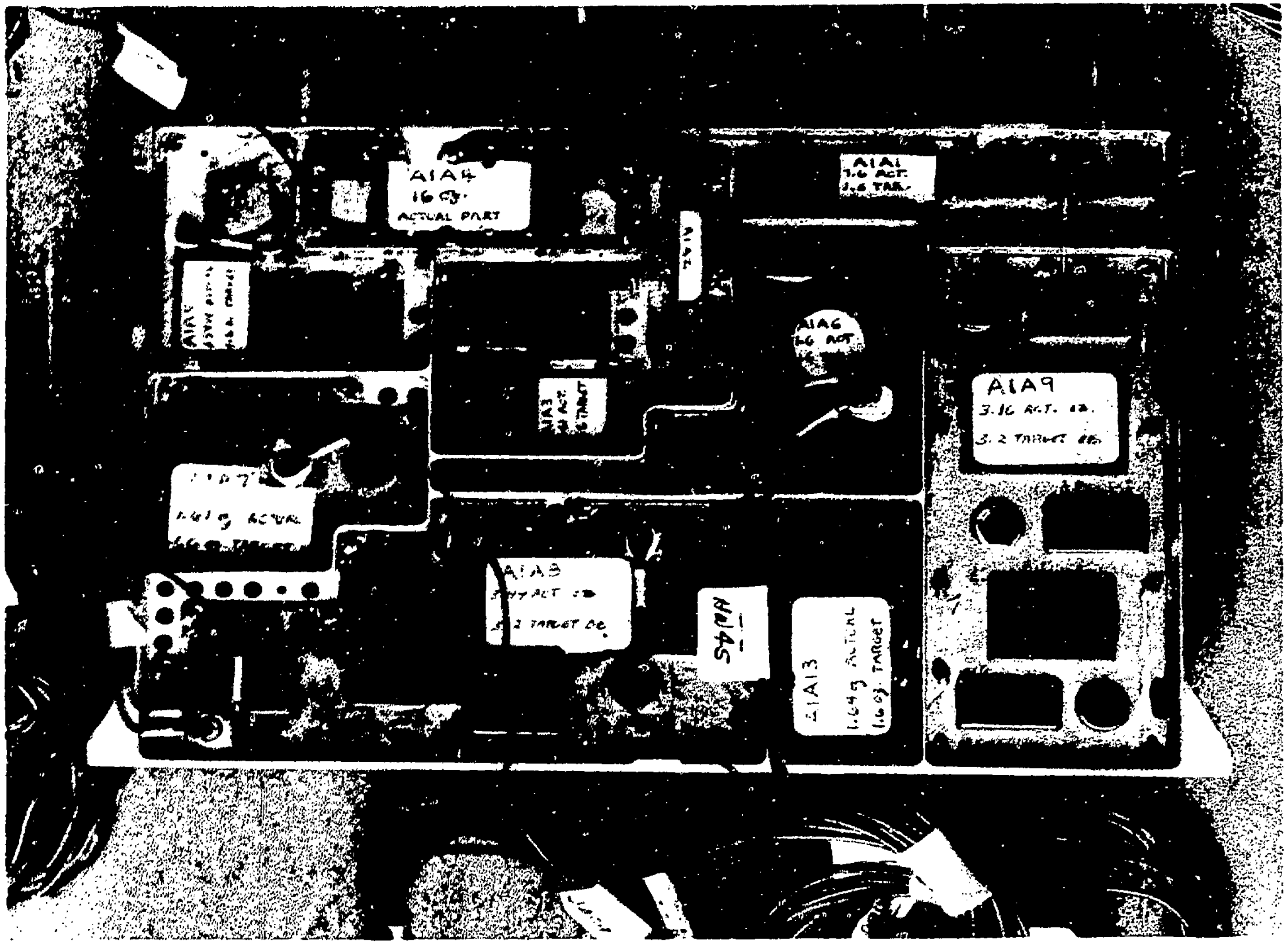


Figure 25. Instrumentation Locations Inside Instrumented Test Unit

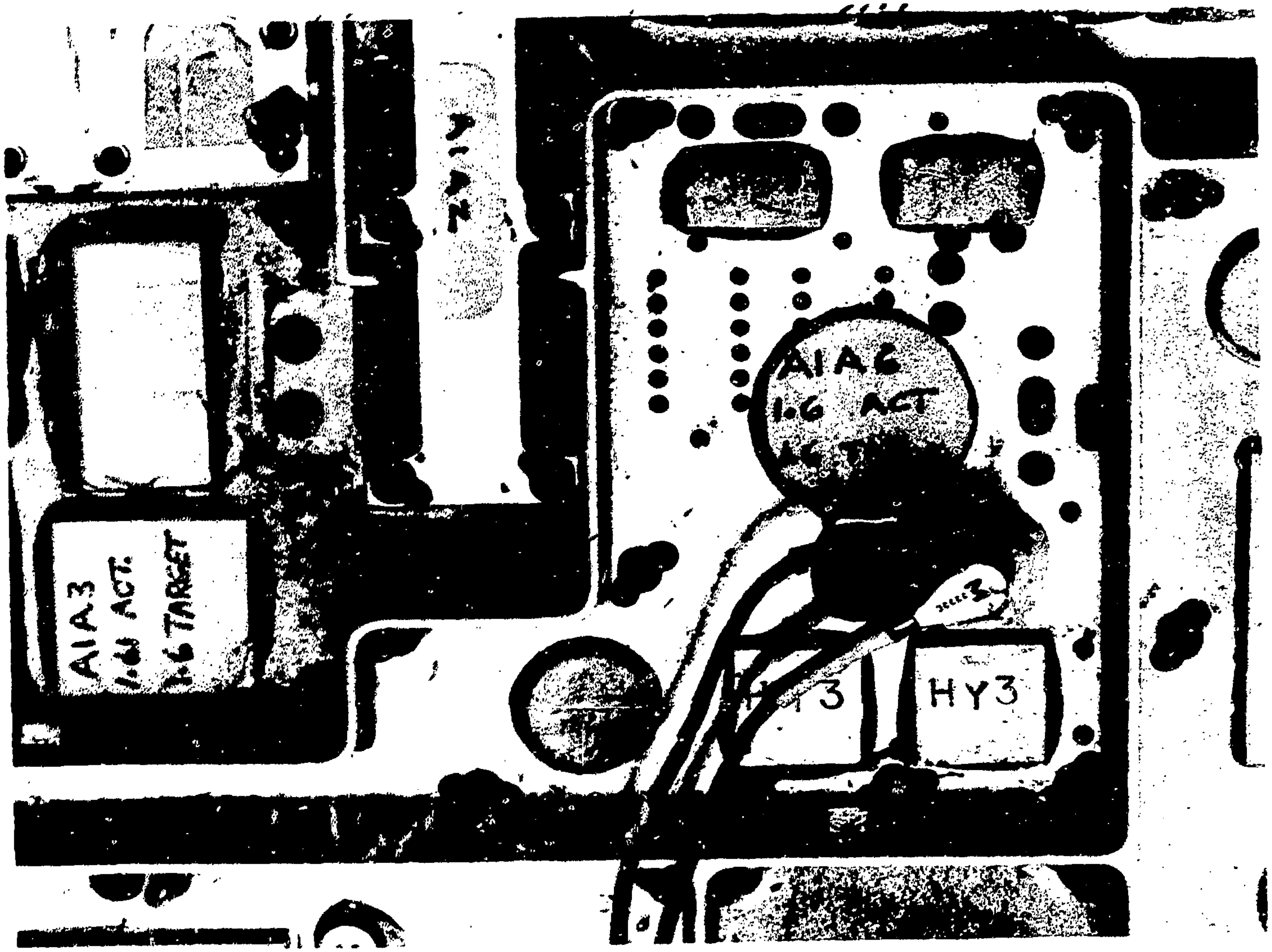


Figure 26. Closeup of Accelerometers Inside Instrumented Test Unit at Mockup of the Circuit Boards

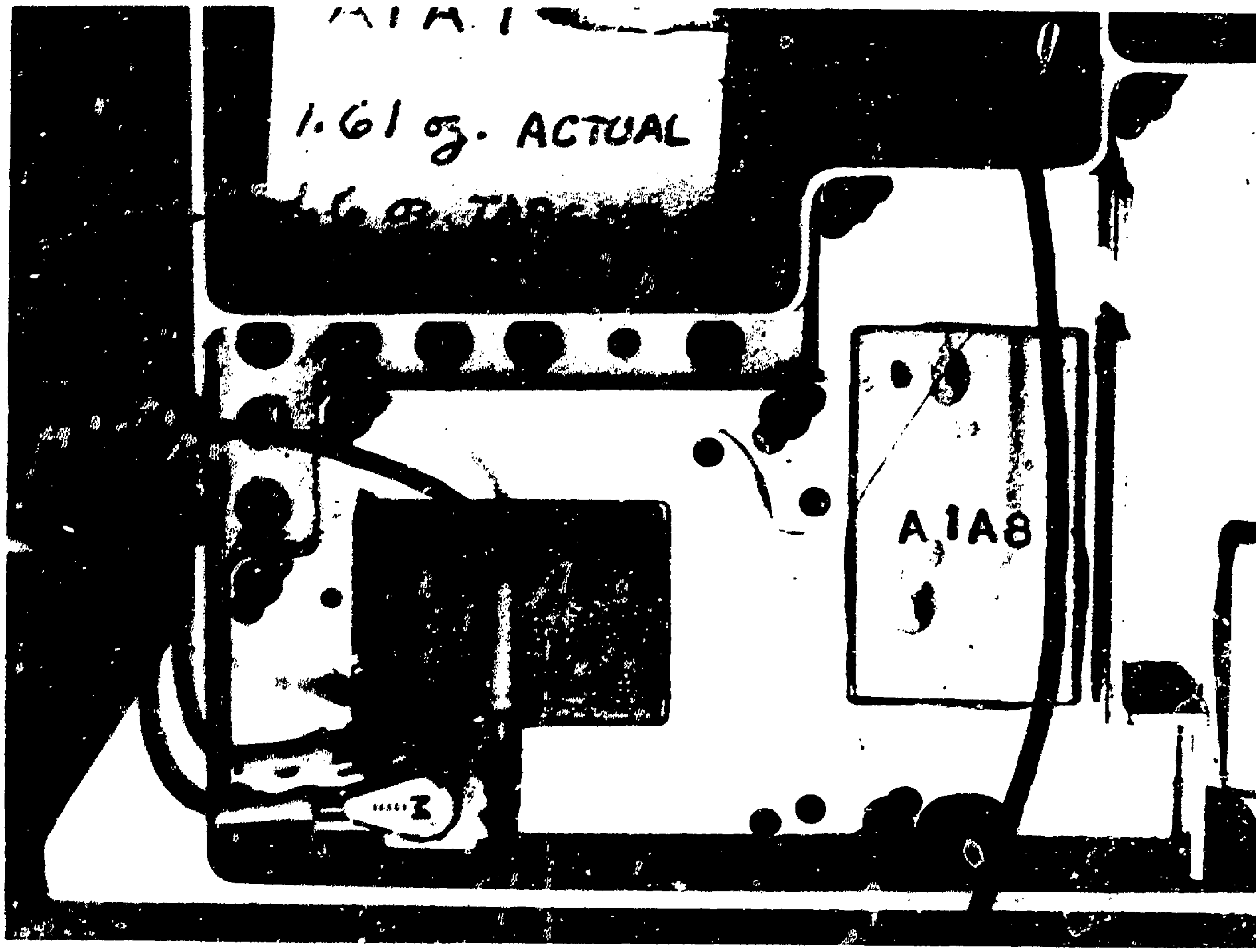


Figure 27. Instrumentation Mounted at the Corners of the Instrumented Test Unit

Discussion

Mr. Mardis (General Dynamics - Pomona Division): I had seen this apparatus before. How much did it cost? How did you establish your material selection and the contact geometry between the hammer and the plate?

Mr. Morse: I don't have an exact answer on the cost. You can see from the material we used to put it together, it is not expensive. However, quite a few dollars were involved in the development work to arrive at the system that is there. We did quite a bit of work on several programs with it, so the cost to TRW, to develop the three particular plates that we showed, probably does not represent what somebody like you might have to do to go into a program now, because you have a pretty good idea of where to start. With regard to material selection, we initially tried steel plates, and they ring much more than aluminum. Probably, if you use magnesium you can get more damping. So, you would have to look at your particular requirements and try to tailor the materials that you want to use toward the spectrum that you have and the levels that you have from the other parameters. "The details are left to the student." About the contact geometry, each of those hammers that you saw are slightly curved so it is not a pointed impact point, but it is rounded in a fairly small area. In many cases we did use a Delrin washer at the impact point. We tried different thicknesses, and different thicknesses gave us different levels. So you would probably end up doing a lot of development work to develop your particular spectrum with that impact point and using very different materials. We used a steel hammer and an aluminum anvil.

Mr. Rosenbaum (General Dynamics - Convair): I guess we at General Dynamics should talk to each other more because we have been using an impact tester for seven or eight years that we made out of an old HYGE machine which we use for the pneumatic hammer. We have done a similar type of testing for electronic components for a long time.

Mr. Morse: Many years ago I was very irritated, and I thought, "Boy, the Navy is really unscientific with their high-impact medium weight shock machine, they just have a hammer hitting a plate." I thought, "How could anybody be so unscientific as to just hit a plate with a hammer and expect to get the right shocks." Yet, the Navy stuck with that system, and the equipment that went into submarines and ships passed those tests, and never had problems. In retrospect, it was not a bad system. They didn't have much data on it. They didn't know exactly why they were doing it, but they were doing it right maybe for the wrong reasons, but probably for a lot of the right reasons. So you have to temper some of the judgements you make as a young fellow as you get older.

Mr. Rosenbaum: In retrospect, talking about what Henry Luhrs was talking about this morning, we found "off-the-shelf" 250-g relays that would barely pass drop-table or drop-tower shocks but they would easily pass orders of magnitude more than that on a resonant-type table like this.

Mr. Morse: I discussed the advantages of the resonant plate in a paper that I presented three years ago. The advantages of the resonant plate are the transmission path and the fact that the shock arrives at the box in a similar way to the real world; you do not overcorrelate the input as you do on a drop table or on a shaker. That is the best portion of the resonant plate simulation that you can do. The mounting impedance is still not good because you are using a plate instead of a honeycomb panel, a spacecraft structure, or what have you. Although the compliance still is not matched, you at least match the transmission path in some respects.

Mr. Rosenbaum: Like you, we have used felt, rubber, Delrin, and all kinds of things by trial and error to vary the widths of the pulse.

Mr. Morse: Again, this just shows a couple of ways to solve problem. You have a problem. You want to perform a shock test. How do you do it? "Resonant plate" techniques are not too bad. You have a few things that you can vary; you can vary some of the parameters and get where you want by using paper for damping or whatever works.

Mr. Dotson (Lockheed): I was involved in the development of this system. One reason we tried this approach was we were trying to develop a system that would cover a particular spacecraft that had many of items mounted on honeycomb panels, and which had very similar characteristics to a flat plate. It was also a system that had a lot of low frequency response. We started off trying to use explosive joints to excite this plate. It was hung vertically at the time, and we found we couldn't generate the low frequencies. In checking one of the accelerometers one of the technicians happened to hit the plate with the sledge hammer, and we got the exact spectrum we wanted. All of a sudden we got real excited about it. So, we laid it horizontally, and because we didn't have an air-impact device at that time, we just dropped an aluminum cylinder down a plastic tube; lo and behold, we were getting all kinds of good results. Then we started varying the parameters; the thickness of the plate, the type of material, the damping material, and the distance from the source. We put it on foam, we put it on sand, and we could vary the spectrum shape widely. It was very successful. I should mention that by putting it on sand, you can move the low frequency modes and steepen the slope. So that is another parameter if you ever need it. As an aside, something that came out of this is that this is a single directional-type device; but many companies who wish to use this device require

three-dimensional equality. We have recently put book-end shelves on the system, and we are getting significant in-plane responses. It is still vertical, but you can rotate the box on the shelf. Another point is when data are taken on a spacecraft, or even on this plate, many times you have a tri-axis accelerometer that is mounted on a little aluminum block or some other type of device. The rocking of the plate, or the rocking of the structure in the spacecraft, can give you what you think is a longitudinal or an in-plane response that is not really there. It is not true in-plane motion. So beware if you are mounting your accelerometers above the neutral axis of the structure or plate, if that is the case, because up to about 7,000 Hz and higher, just the rocking effect is equal to the normal response. Once we went back and looked at all of our spacecraft data, we realized that all the enveloping of what looked like in-plane response was really a rocking effect due to normal response due to the bending waves. So, I think many of the three-dimensional spectral requirements came from non-true three-dimensional effects.

Mr. Morse: That is a good point. I have to agree with what is mentioned in the lead-off chart, that our requirements are only for a single axis. In a previous project we had to meet the same shock requirements in all three axes. If you look at the way components are mounted in a spacecraft, shock really comes to it that way. You don't get the same response in all three axes. You get a major response perpendicular to the plane in which the component is mounted. You don't get the same in the other in-plane axes. I think that is well recognized in vibration because many vibration specifications now require different levels in-plane, and normal to the mounting plane. I think that eventually should be the same for shock requirements.

Voice: The last two or three minutes of discussion satisfied the comment I was going to make. It was about the compromise that one might have to accept in a lateral axes, but I thought Ron addressed it real well by saying with book shelf-type fittings you can get the lateral axes as well.

The Controlled Response of Resonating Fixtures Used to Simulate Pyroshock Environments

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ABSTRACT

This paper describes the test techniques used at Sandia National Laboratories for simulating pyrotechnic shock on components. It is a "Resonating Fixture" approach, sometimes known as a hammer test. This paper brings together information that is available separately in the literature, and adds details, not previously published, which should enable the reader to reproduce these techniques for their own use.

When working with some pyrotechnic device, or some other impulsive stimuli on a real structure, (Fig. 1), experience indicates that somewhere near that pyrotechnic device, very high g levels; perhaps greater than 100,000 g's, and very high frequencies, perhaps greater than 50-100 kHz, exist. In this region (Region I, Fig. 1), the shock is best described in terms of stress wave propagation as opposed to structural response. I describe this region as the "material response to the stimuli." In most structures, somewhere remote to the pyrotechnic device, g levels tend to be lower, typically less than 20,000 g's, and dominant frequencies are also lower. Those dominant frequencies are on the order of 1,000-10,000 Hz. The response of the structure in this region (Region II, Figure 1) is dominated by the structural response of the entire structure. Most of the pyrotechnic shock environments encountered at Sandia are of the Region II type. This Region II environment can be adequately simulated with mechanical impact test techniques; a number of these mechanical impact techniques are described in the literature.

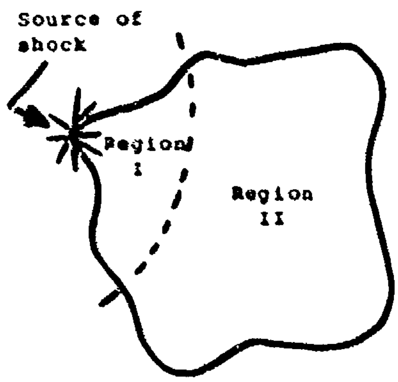

Figure 1. Two distinct regions of pyrotechnic shock.

Figure 2 shows design philosophies for some of these impact test techniques. In Figure 2a, the test component is attached to the actual structure it will be used in. The test structure is struck in a trial and error fashion until a response which satisfies the test requirement is obtained.

Another test technique (Figure 2b) also uses a trial and error method of determining the response at the test item. Instead of using the actual structure which may be very complex, a test fixture of a simpler geometry, such as a plate fixture, is used.

Sandia has many different test items with various shock spectrum requirements, as opposed to a production agency that might have only a few test items with the same requirement. We must have a test technique where we can easily develop a variety of shock spectra without an elaborate effort to design a very specific test apparatus. Figure 2c shows how this is done. We have a test fixture to which we mount the test item. That test fixture is struck with either a pendulum hammer or an airgun-fired projectile. That test fixture is analytically designed so the response of the test fixture and the test item are know prior to performing the actual test.

The fixture response is some function of the test item material and geometry, the test fixture material and geometry, the impact forcing func-

tion, and its location and direction. This could be a very complex analysis, but fortunately the analysis can be simplified in several ways. First, a simple test fixture, e.g., a beam, thick plate, or bar fixture whose modes are simple and a known function of geometry can be selected. Second, the fixture can be made relatively stiff and massive so its response is essentially independent of the test item to which it is mounted. Thus, the test item can be neglected and the solution to the analysis decoupled. Experience indicates we can assume that the impact is approximately a half sine pulse with variable amplitude and duration.

The two fixtures selected for this purpose are a bending plate fixture and a longitudinally resonant bar fixture, hereafter referred to as a Hopkinson bar. The bending plate fixture is a square plate whose dimensions are L by L by thickness T (Fig. 3). It is struck on the center of one side, and the component is mounted on the opposite face in the center of the plate. The first bending mode of the plate is the one which we attempt to use. This is approximately given by equation 1.[2] For this case, the component, as shown in Figure 3, is located at an anti-node for the first bending mode. The response we excite is perpendicular to the base of the component for this configuration.

The Hopkinson bar, (Fig. 4), is utilized in a similar manner, but impact occurs on one of its ends, thus exciting that fixture into its longitudinal modes of vibration. Those modes are calculated from the one-dimensional wave equation. The result is given by equation 2.[3] In the configuration illustrated, the input to the test item would be tranverse to the base.

The method of using the first modes of a plate fixture or a Hopkinson bar to simulate pyrotechnic shock was first proposed by Bai and Thatcher.[1] In their paper, they selected a pair of fixtures, a bending plate fixture and a Hopkinson bar fixture, which have the same first modes. They tested the component perpendicular to its mounting direction on the bending plate fixture and the two transverse directions on the Hopkinson bar fixture.

These fixtures are designed in a simple way, so that their structural mode(s) match the frequency content of a given test specification (i.e., shock spetrum). Figure 5 shows a normalized log-log shock spectrum of a single degree-of-freedom, damped linear oscillator; while not exactly drawn, the character is shown. If the first mode of one of these fixtures is excited, the resultant shock spectrum would resemble that in Figure 5, and the time history would resemble the inset drawing.

A shock spectrum from an actual pyrotechnic shock is shown in Figure 6. The shock spectrum from a single degree-of-freedom oscillator can be overlayed in such a manner as engineering judgment

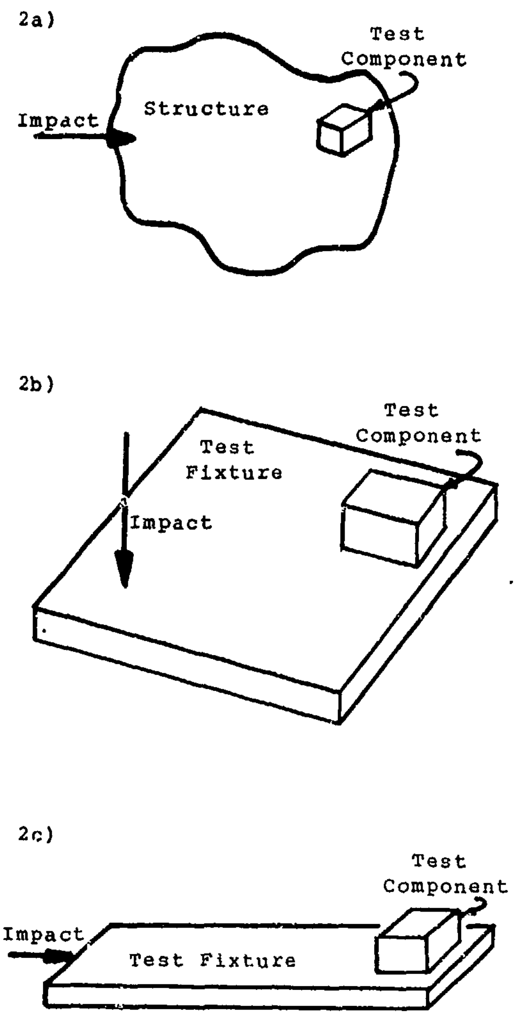

Figure 2. Test design philosophies.

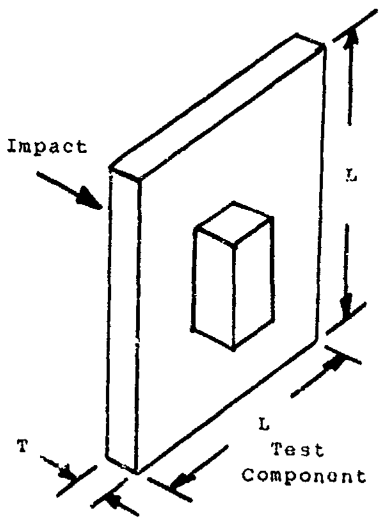

Figure 3. Bending plate fixture.

would dictate to be the best envelope. Figure 7 illustrates this envelope. It turns out that a fixture resonance of about 2,000 Hz with a peak acceleration of about 2000 g's is needed to simulate this particular environment.

$$f_1 \simeq 22.4\sqrt{\frac{ET^2}{12L^4\rho}}$$

where E = modulus of Elasticity

ρ = density

T = plate thickness

L = plate length and width

Equation 1

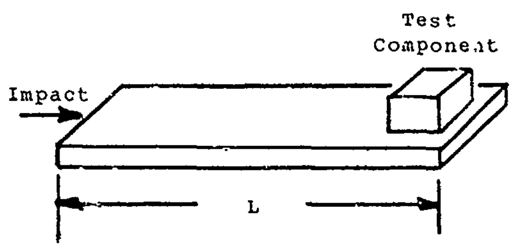

Figure 4. Hopkinson bar fixture.

$$f_n = \frac{nc}{2L}$$

where n = 1, 2, 3...

c = wave speed in bar

L = bar length

Equation 2

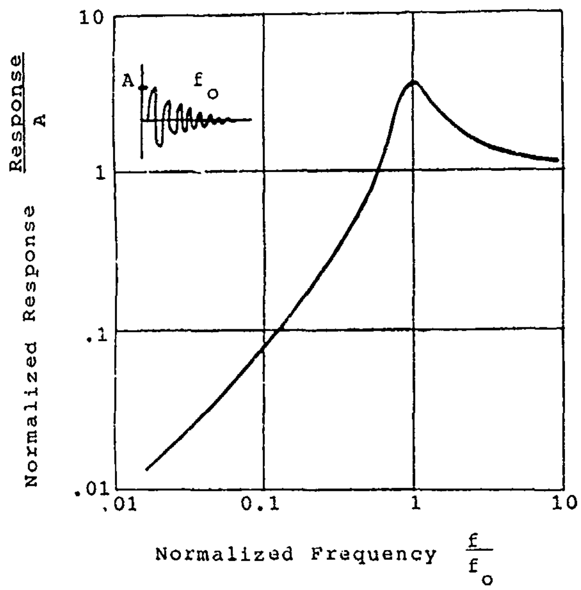

Figure 5. Normalized shock spectrum of a damped linear oscillator.

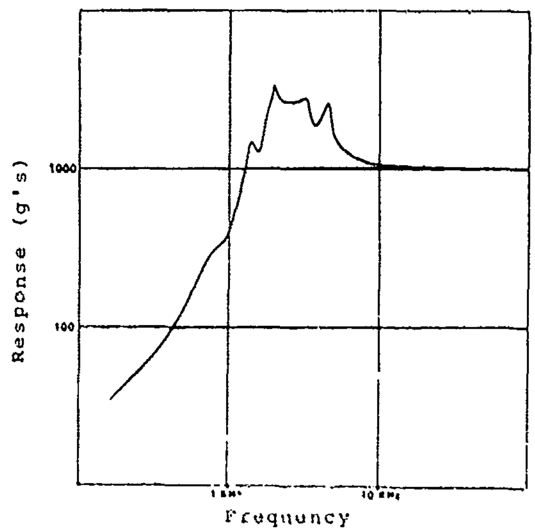

Figure 6. Shock spectrum of actual pyrotechnic shock.

The first modes of these fixtures are used since the response shock spectrum is approximately known. The dimensions of these fixtures are designed so their first modes correspond with the peak on the shock spectrum. This method applies to a somewhat limited class of pyrotechnic shock environments that have a shape similar to that one-dimensional decayed oscillator. Most actual environments seen at Sandia Laboratories fit that shape very well. Once the fixture geometries are selected and their sizes determined, their modes of vibration are fixed. We then impact the fixture in order to excite the first mode. This is done by controlling the amplitude and duration of the input pulse which is applied by a hammer or projectile. For example, a beam with a first mode of 1,000 hz, requires an input pulse duration of about one millisecond. The amplitude of

that pulse is simply varied by increasing or decreasing the impact velocity; the duration is controlled by various shock programmers. Sometimes an elastic programmer (Figure 8) is used, which consists of a piece of Delrin plastic

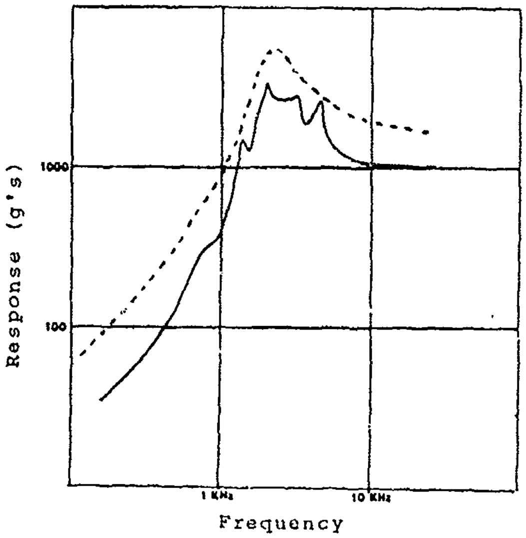

Figure 7. Previous shock spectrum showing envelope.

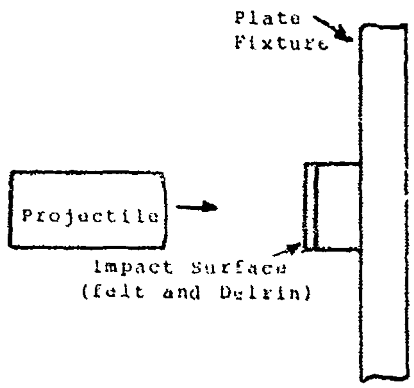

Figure 8 Detail of typical impact surface.

and a piece of felt. This is typically for plates with a low natural frequency (<1000 Hz). For higher frequencies, a metal-to-metal impact is used. In these cases, the programming material is usually a piece of aluminum. The aluminum is indented with a projectile or hammer which has either a spherical or conical nose. The duration is varied by changing the spherical radius or cone angle. For example, if the cone is made sharper, the impact duration would be longer. With only minimal trial and error, the impact duration can be lengthened or shortened so that the first mode of the plate or a beam fixture is excited.

Fixture damping is another parameter which needs control. These structures are fairly uniform, continuous media, hence they have very little damping of themselves. A component mounted to that structure increases the mechanical damping, however, these fixtures still resonate for hundreds of milliseconds. This is not desirable because the actual pyrotechnic shock environment typically lasts less than 20 milliseconds. These fixtures can be mechanically dampened by clamping various bar or plate materials to the fixture itself. These bars tend to lower the first mode of the fixture by not more than 20%, which is usually acceptable. This simplifies the analysis since the damping clamps do not have to be accounted for when calculating the first mode frequency of the fixture. For example, a damping arrangement on the bending plate fixture as shown in Figure 9 is a square aluminum bar clamped to two edges of the plate with C-clamps or bolts. The same thing can be done for the Hopkinson bar by clamping a small plate stock on its impact end (Figure 10). The small x's indicate the presence of either a bolt or a C-clamp attachment point. The damping may be increased (or decreased) by using more (or fewer) clamps. The maximum number of clamps needed does not greatly affect the calculated first mode of the structure.

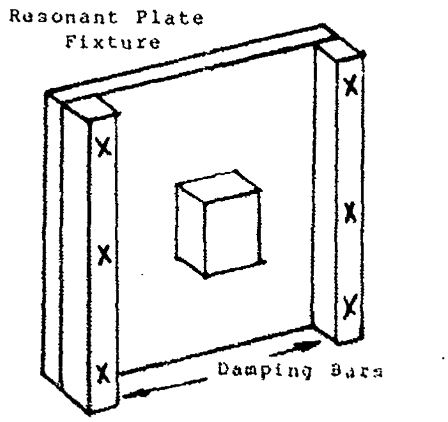

Figure 9. Damping bars added to bending plate fixture.

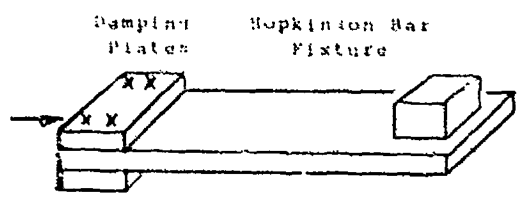

Figure 10. Damping plates added to Hopkinson bar fixture.

Controlling the response of the Hopkinson bar fixture with these damping clamps is the subject of a paper presented by the author at the 1985 IES Annual Technical Meeting.[4] The basic result of that paper states "Masses clamped at the nodes of the ith mode cause the response to be dominated by that ith mode." For example, the nodes for the second mode of the Hopkinson bar occur at L/4 from each end. Figure 11 shows a pair of masses (plates) clamped at the nodes of mode 2 for a Hopkinson bar. If that plate is impacted longitudinally with the appropriate duration pulse, the fixture can be excited into

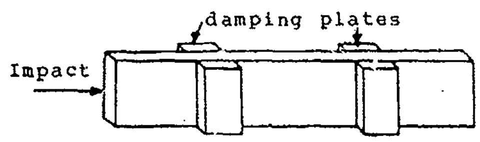

Figure 11. Damping plates positioned on a Hopkinson bar, so that the second mode is dominant.

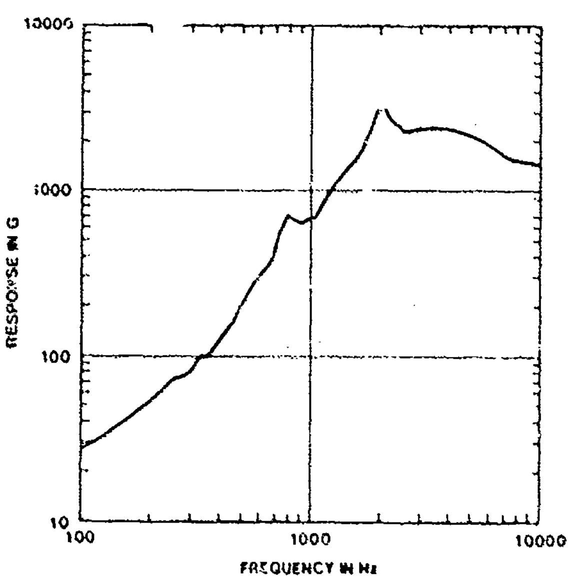

Figure 12. Shock spectrum showing a dominant mode 2 response.

its second mode response. The Hopkinson bar used consisted of a two-inch by ten-inch by eight-foot long aluminum bar which was the basic test fixture. Figure 12 illustrates the shock spectrum of such an arrangement. The first mode of that fixture is 1,000 Hz. Note that the 2,000 Hz second mode is dominant and the first mode is suppressed and shifted to about 800 Hz. With this method, the 1st, 2nd, or 3rd mode of this Hopkinson bar can be selectively excited. At higher modes the nodal spacing becomes closer and there is a tendency to overlap different nodes with the clamps placed on the bar.

The techniques described provide a very practical means of simulating pyrotechnic shock of the structural response type (Region II of Fig. 1). These techniques eliminate most of the trial and error required by other test methods.

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APPENDIX

Transcript of discussion following presentation of this paper at the 56th Shock and Vibration Symposium.

Mr. Safford (Agabian Associates): About 1971 or 1972, Pat O'Neil and Chuck Tierman or TRW did some hammer-type impact tests on long bars where they hung weights that were gasketed with an elasto-plastic material. They were looking to attenuate the shock front. They did a lot of very nice work. It might be applicable. It is published material and easily accessibile. It is a nice little article; it might help you.

Mr. Galef (TRW): It looks like you are hitting that free-free plate with a rather good sized mass going at a rather good speed. You are also exciting the rigid body mode in addition the the first mode that you want to excite. I believe you are applying a test which is quite unrealistic in comparison to pyrotechnic shocks. You will have much more energy than you want at the low frequencies unless you somehow restrain that plate.

Mr. Davie: It turns out that the velocity change of the plate, which is very massive, is very small. The hammer may be large by what your experience indicates, but the velocity change of the plate due to the impulse is fairly small. You can see that by looking at the shock spectrum that we have generated from those techniques. The velocity change is usually well under ten feet per second, perhaps even less. It is true the velocity change might be higher than what you would see in an actual pyrotechnic shock environment; however, as far as the shock spectra is concerned, if you had an undesirably high velocity change, that would be indicated in the shock spectrum, and that is not the case.

Mr. Powers: I really appreciate Neil's idea of defining two distinctive areas. I think many people do not realize that there really are two distinctive areas in pyrotechnic shock. When you are very near the source, we make comments like, "The shock response spectrum in all three axes is approximately equal." We also have to realize about the comment about three accelerometers that what we are looking at are all mounted on a little one-inch block. However, as you travel further away from this Zone 1, the basic structure is no longer excited primarily due to the speed of sound, or through the longitudinal modes of the structure. It is excited more in the classical modes of vibration and dynamics. As I said earlier today, if you go away from a source, I don't really think it would make much difference what you hit the aft end with. By the time you are far from the source, if you monitor on a telemetry rack, it will resonate at its own natural frequency.

MULTI—AXIS TRANSIENT SHOCK SIMULATION USING MECHANICAL PULSE GENERATORS

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Pyrotechnic shock is represented by a very short time duration, accelerations into the 1000 g levels, and a wide frequency band up to 10 khz. The current technology of laboratory simulation test machines is hard pressed to repetitively meet these requirements in one axis and much less so in two or three axes simultaneously. A bi-axial transient shock machine under development for the U.S. Army ERADCOM is described and shows potential to adequately meet these pyrotechnic requirements in two-axes with extension to three-axes.

* Now Professor, Mechanical Engineering, Northrop University

When it comes to pyrotechnic shock, I am a latecomer. Some years ago, it was postulated that a series of force pulses could be used to stimulate motions in structures. If a train of force pulses can be configured, for time duration, the onset time and the amplitude of each and every pulse and if the dynamic characteristics of the structure, (the transfer impedance) can be determined (analytically or by testing), can the response motion that would duplicate the test events or real events in practice be produced? Computer simulations were made from which a procedure was evolved that showed errors within five percent error with respect to the expected motion. This led to the building of a series of pulse machines that ranged from metal cutting, (where metal chips are cut) to using cold gas pulse generators, to generating pulses using single shot chemical rockets. Machines have been built that go down to a few hundred pounds of force, up to a hundred thousand pounds of force and designs that look very feasible, to up to a million pounds of force.

Much of this work was accomplished for the U.S. Army, the Department of Energy, and for the National Science Foundation. The current project is for the U.S. Army Electronics Research and Development Command. The U.S. Army is very concerned about communications equipment since the army of the future will be nuclear, and must withstand nuclear shock as well as high explosive shock. Figure 1 shows a conventional two and a half ton truck with a recently developed hardened shelter made of Kevlar. The truck has guy wires to prevent overturning and is tested in various configurations under blast loads by high explosive events. Kaman Sciences is performing very elegant finite element studies using ADINA, to predict what happens to the structure when the blast wave hits. The army requires a machine that can be used to simulate the predicted response of C³I electronic equipment housed inside the shelter. This machine will be used for qualification tests and acceptance tests on every piece of equipment for assurance of battle hardness. Every time that equipment is returned to the depot for repair, it is run through the test, at a lower level, of course. The way this technique works, given a criteria function, or an objective function (motion-time history), impedance measurements or calculations are made so that the structure and the load impedance of this system are known. This computer model is stimulated with force pulses, run through step-by-step to obtain a response, then through an error function, do the optimization and then come back and correct as shown in Figure 1. The pulses are single-sided or push pull (attached to opposite sides of structure). The optimization program took several years to develop, and it is a random search technique. It is rather computer time-intensive.

The other criteria the army has is that they do not put much stock in shock spectra; they prefer time histories as a governing yardstick. They feel if you are in the range of the time durations, the general envelope of the time-history peaks and the frequency bandwidth, test simulation is more realistic. Failure is largely non-linear, therefore one is pretty well tied to the time-histories expected or a reasonable class of time-histories. Shock spectra and Fourier spectra are used and help support the testing requirements given the constraints imposed by the time-histories.

Figure 2 is typical of test data taken during a high explosive test. It shows the response in the middle of a rack of equipment. Accelerations can range up to about 12 to 15 hundred g's. The time durations go out beyond 200 milliseconds. Motions are largely horizontal and vertical with some horizontal motion normal to blast direction. Frequencies up to 5 khz have been recorded.

There are several ways to implement pulse generation, and Figure 3 shows a metal cutting technique. A cutter moves across the mandrel, and the shape or profile of the metal to be cut and the velocity of cutting determines the force-time-history. The Waterways Experiment Station, co-developers of this system, call the metal

elements to be cut "nubbins". The grooves cut in the nubbins transmit forces to the test article.

Aluminum "nubbins" for the Department of Energy were built which produced 50,000 pounds of force in 40 milliseconds, and with six nubbins in a series, a two-story building as big as a football field was excited. The mode shapes of the building were extracted from the building motion. If the "nubbins" had been made of steel, about 100,000 pounds of force would have been produced.

Figure 4 shows a schematic of the system for the Department of the Army (the Harry Diamond Laboratories, and the Ballistic Research Laboratory). The test article is held in a modified equipment rack which has a force link, a row of "nubbins" and the cutter. A hydraulic ram is employed to drive the "cutter" but gas rams have been used in the past for much higher speeds. Speeds on the hydraulic ram run about 100 inches per second, but about 1,000 inches per second can be obtained with a gas driver. These higher speeds permit very high frequency excitation. The system is biaxial but a third axis can be added if field tests so justify.

Figure 5 is a picture of the machine that is currently in development. The hydraulic ram, the cutter, the mandrel with the "nubbins", what is called a "quadrapod" that carries the load into the test structure, and equipment under test comprise the test machine. The same is repeated for the vertical direction.

For a high impedance load, like a wall, rectangular cuts are obtained (Figure 6), but if against a low impedance source, then the load and source interact and you do not get perfect cuts unless you compensate. Compensation both for the load impedance and for the source impedance with the algorithm can be made so as to produce more optimal responses.

We were fortunate to have a peer review when we started looking at this machine for a possible application to pyrotechnic shock, both by the Aerospace Corporation and TRW. Figure 7 shows a series of "nubbins" that run about one-half a millisecond up to about two milliseconds on the test machine (Figure 5). They are geometrically spaced so as to get a fairly decent frequency spectrum. The test was only run in the horizontal direction which is given by an accelerometer on the equipment under test. This series of forces were measured by the load cell, and the test racks are interacting with the input forces. What led into this consideration for pyrotechnic shock application was that in one of the calibration tests using an arbitrary series of pulses, an acceleration-time-history was obtained which appeared to be a credible simulation. These data are shown in Figure 8 with peak accelerations of 1200 g's and a time duration of 40 milliseconds. Frequency ranges to 10,000 Hz as shown in the Fourier magnitude plot.

Our plans are to start looking into simulating pyrotechnic shock. Initially, we are not going at it with the approach to meet a shock spectrum; it would be preferable to first get pulse trains to match the predicted equipment acceleration time-histories as closely as possible and then look at the shock spectra and tweak beyond that.

ACKNOWLEDGEMENTS

The development of these pulse generating machines is a result of joint and collaborating effort with Professor S.F. Masri, University of Southern California; R.E. Walker, J.P. Pickens, G.T. Easley, and R.D. Crowson, U.S. Army Waterways Experiment Station; R.M. Lingebach and L.J. Belliveau, U.S. Army Harry Diamond Laboratories; Dr. W.J. Schuman, Spectrum International; C.C. Huang (retired), Corps of Engineers, Huntsville; and Dr. J.B. Scalzi, National Science Foundation.

DISCUSSION

Dr. Rubin (The Aerospace Corporation): With the time-history simulation you will run into the problem that we run into all the time. The army requirement is for a specific truck, a specific rack, a piece of equipment at a specific location, and you've got a time-history. We do not have that. Equipment can move around, it can be on this spacecraft or that spacecraft. There is no single time-history. When you look at it from that standpoint you are forced to a spectrum type of description. You can think about it in the time-history domain and start that way; but, when you have the variability in where you can locate equipment, then the time-history does not mean anything except for one specific case.

Dr. Safford: Yes, I would agree with you. I think the correct approach is to look at the time-history and generate the first spectra, and then start looking at the variation around there. I think that would probably be a solution with the people that set the criteria.

Voice: I do not believe that I understand it very well, but are you able to generate negative forces with that system?

Dr. Safford: Yes, we just put the pulse machine on the opposite sides of the structure so we have two of them in there, and they pull. They both pull, but they are time sequenced to achieve positive and negative forces.

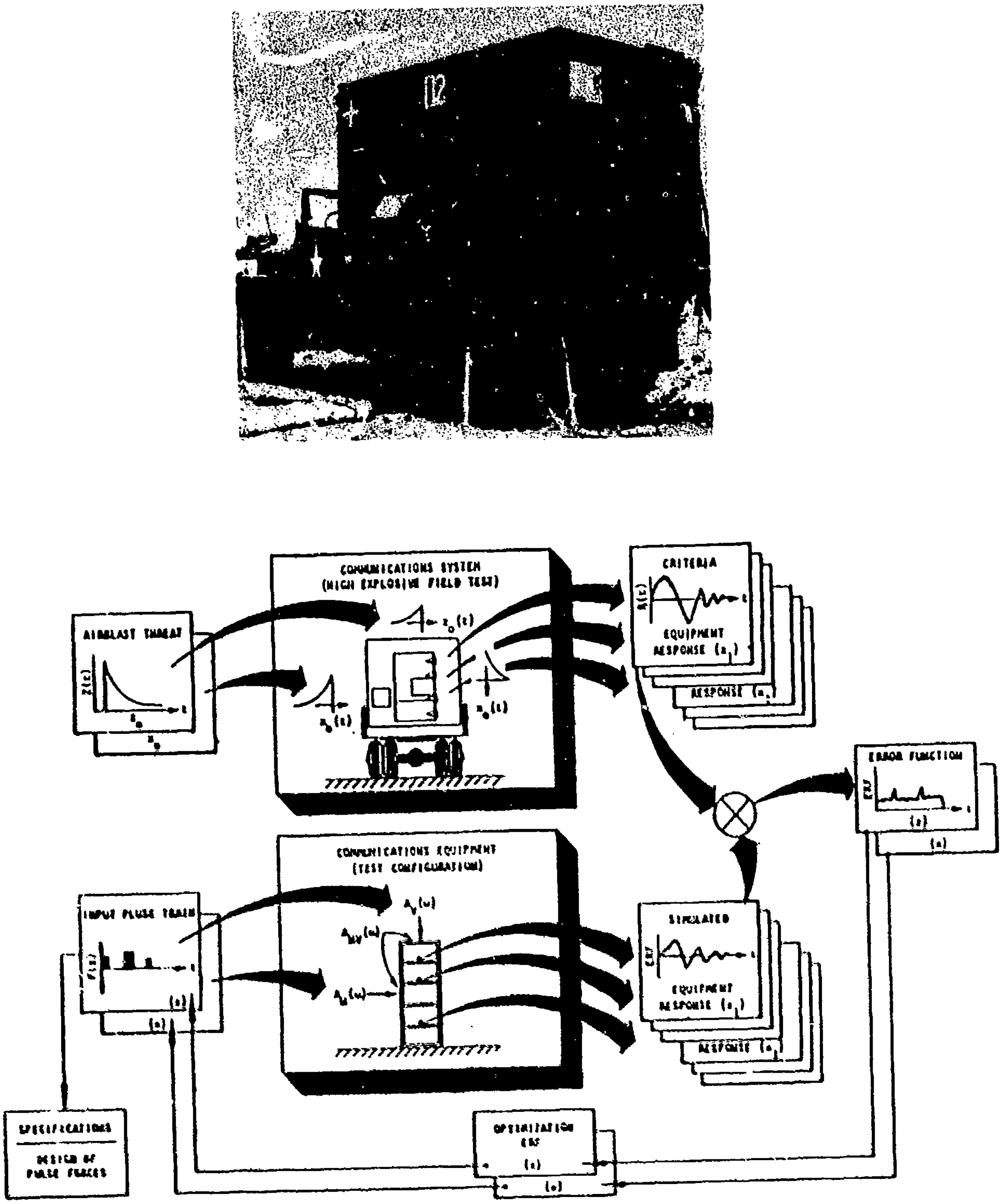

FIGURE 1. OPTIMIZING PROCEDURE FOR PULSE EXCITATION IN LABORATORY OF COMMUNICATIONS EQUIPMENT TO MATCH MOTIONS INDUCED BY AIR BLAST LOADS ON TWO AND ONE-HALF TON COMMUNICATIONS TRUCK

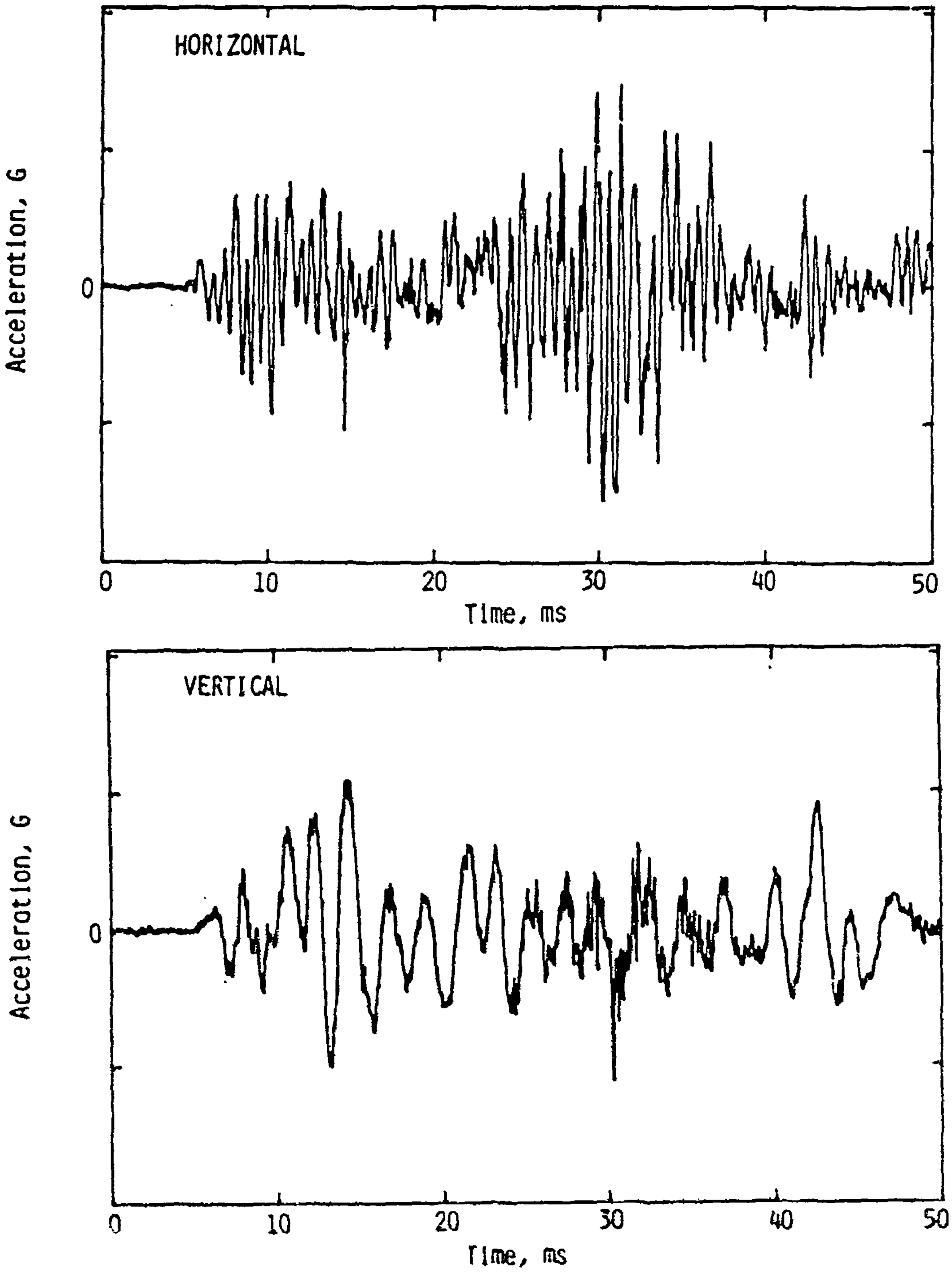

FIGURE 2. TYPICAL ACCELERATION-TIME HISTORIES OF EQUIPMENT TRANSIENT SHOCK MOTIONS AS OBJECTIVE FUNCTIONS TO BE MATCHED BY PULSE PROCEDURE OF FIGURE 1

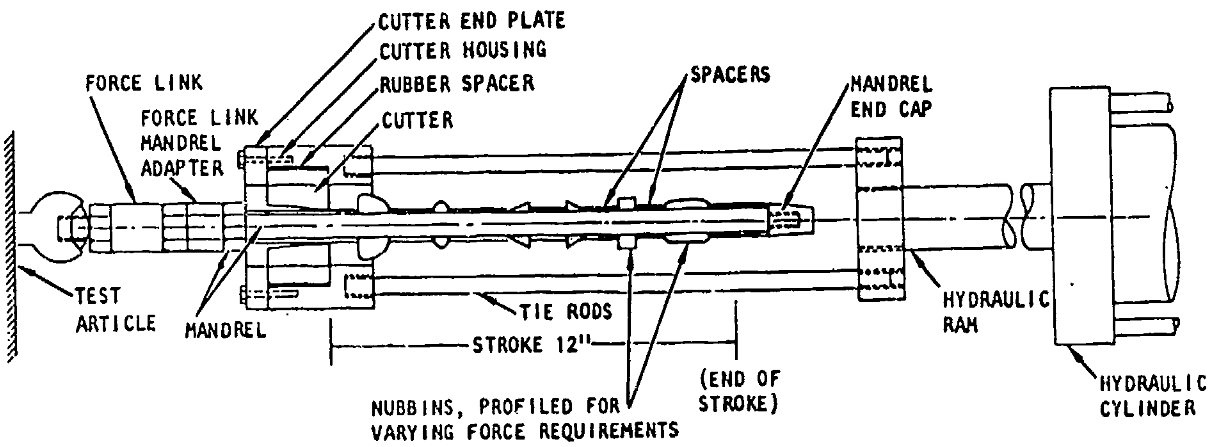

FIGURE 3. SCHEMATIC OF FORCE PROFILE GENERATION BY METAL CUTTING

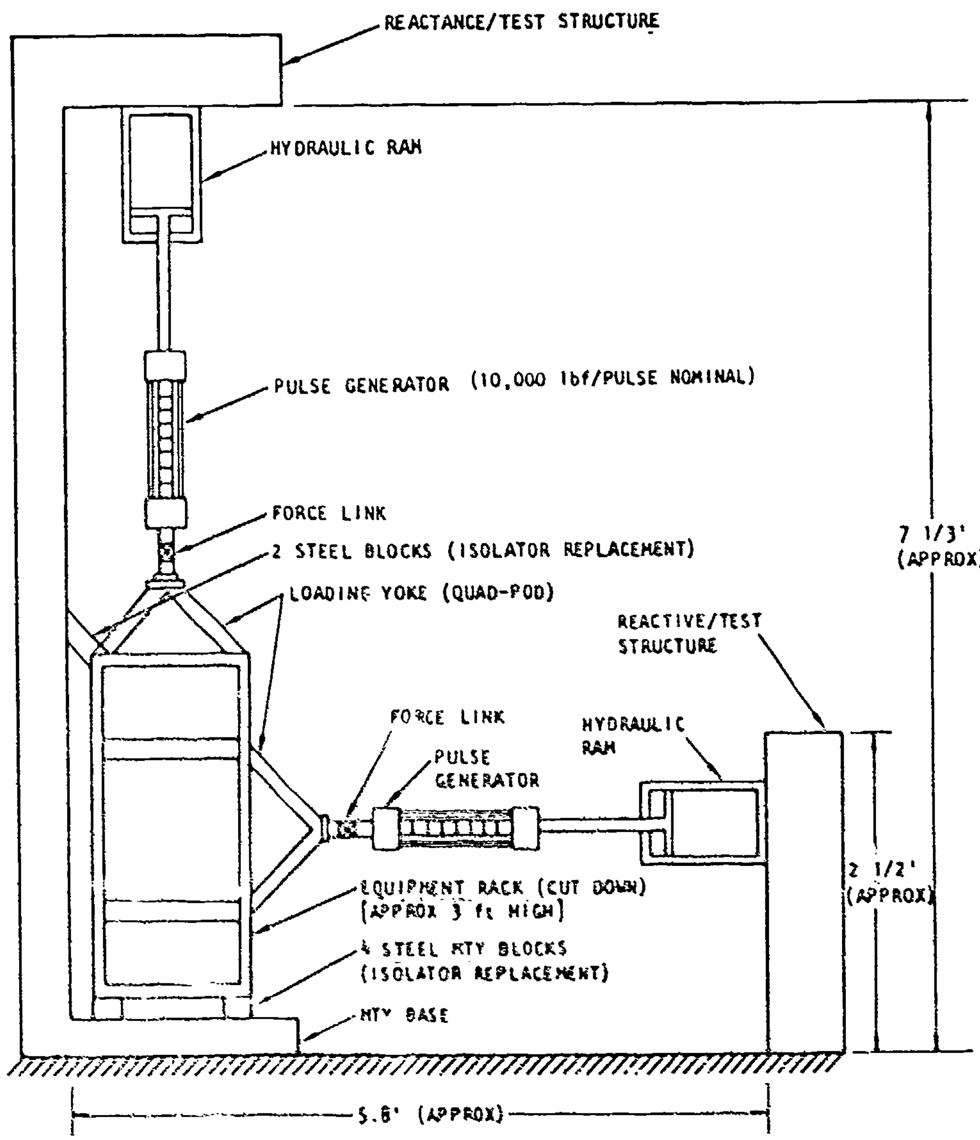

FIGURE 4. SCHEMATIC OF BIAXIAL COMPONENT SHOCK SIMULATOR I (Typical performance 200 m sec duration, 3000 g peak, 0 to 10 kHz)

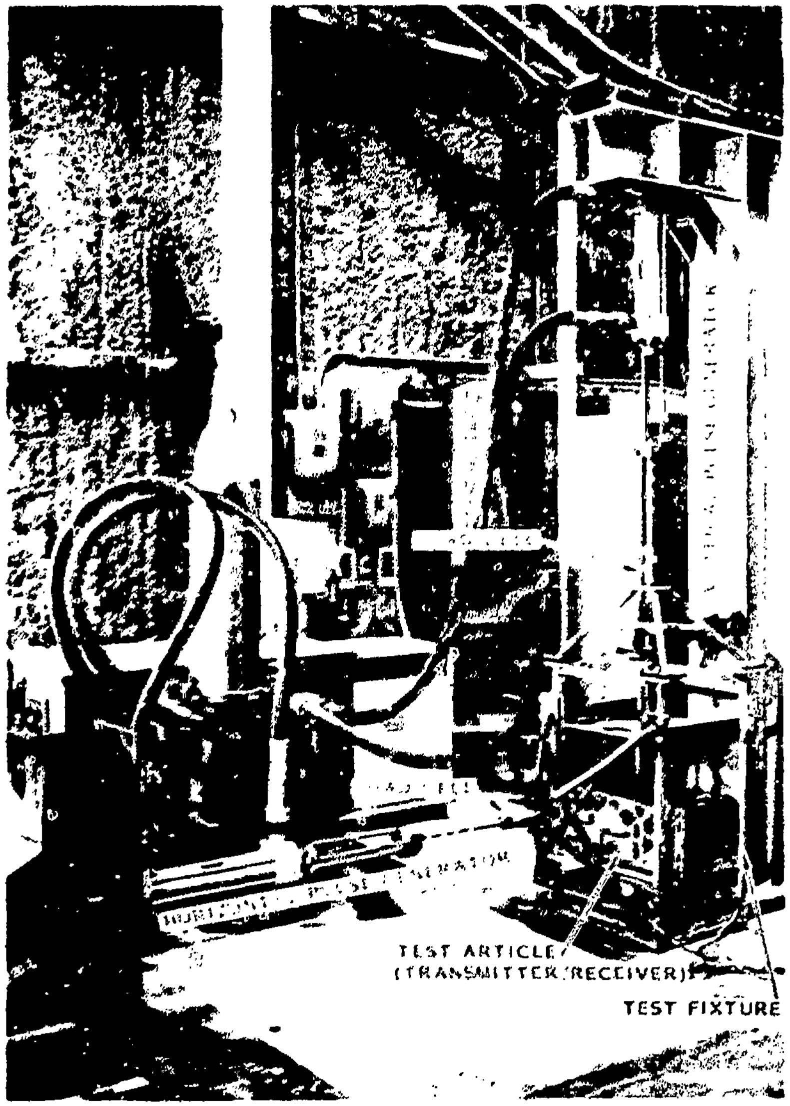

FIGURE 5. BIAXIAL COMPONENT SHOCK SIMULATOR I (CSSI)

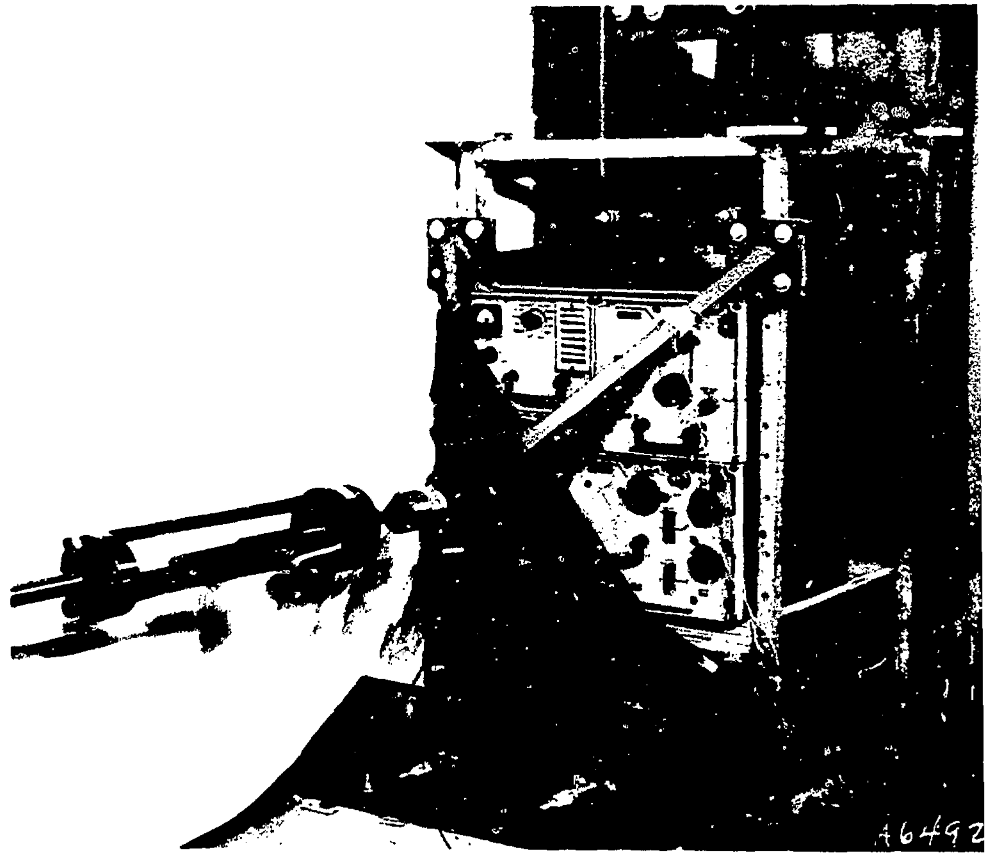

FIGURE 6. VIEW OF COMMUNICATIONS EQUIPMENT UNDERGOING TEST

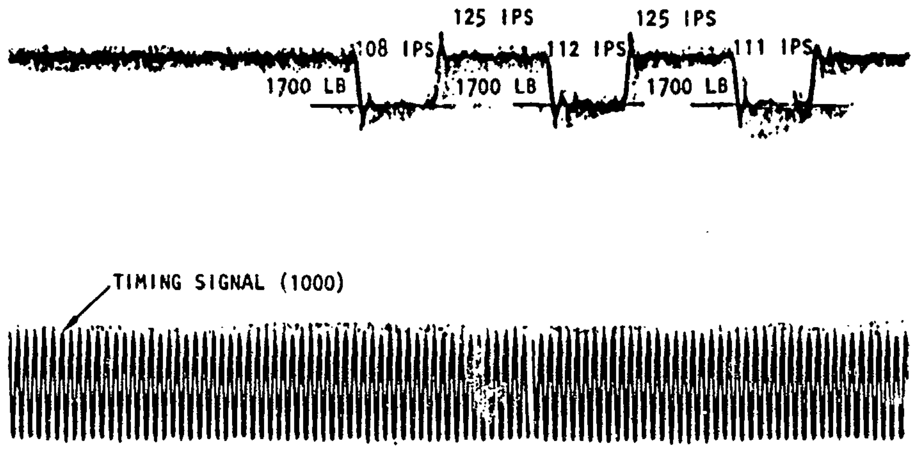

FIGURE 7. PULSE TRAIN CALIBRATION TEST AGAINST A HIGH IMPEDANCE LOAD (4 cuts, each 1/4 in. wide x 1 in. long x 0.006 in. deep, aluminum nubbin)

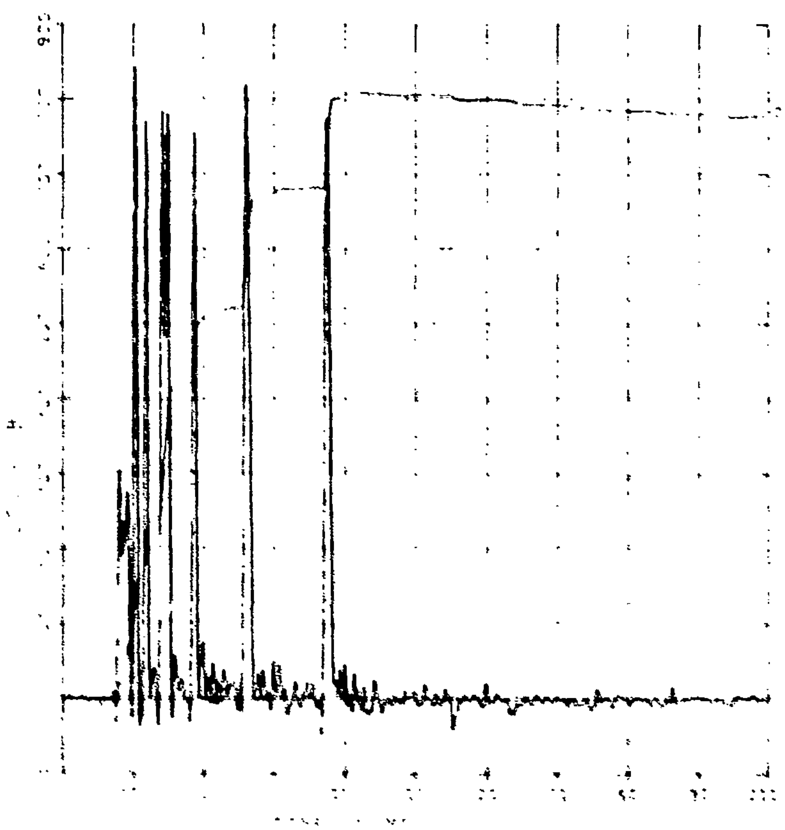

FIGURE 8. TEST PULSE TRAIN FOR CSSI FIGURES 5 AND 6

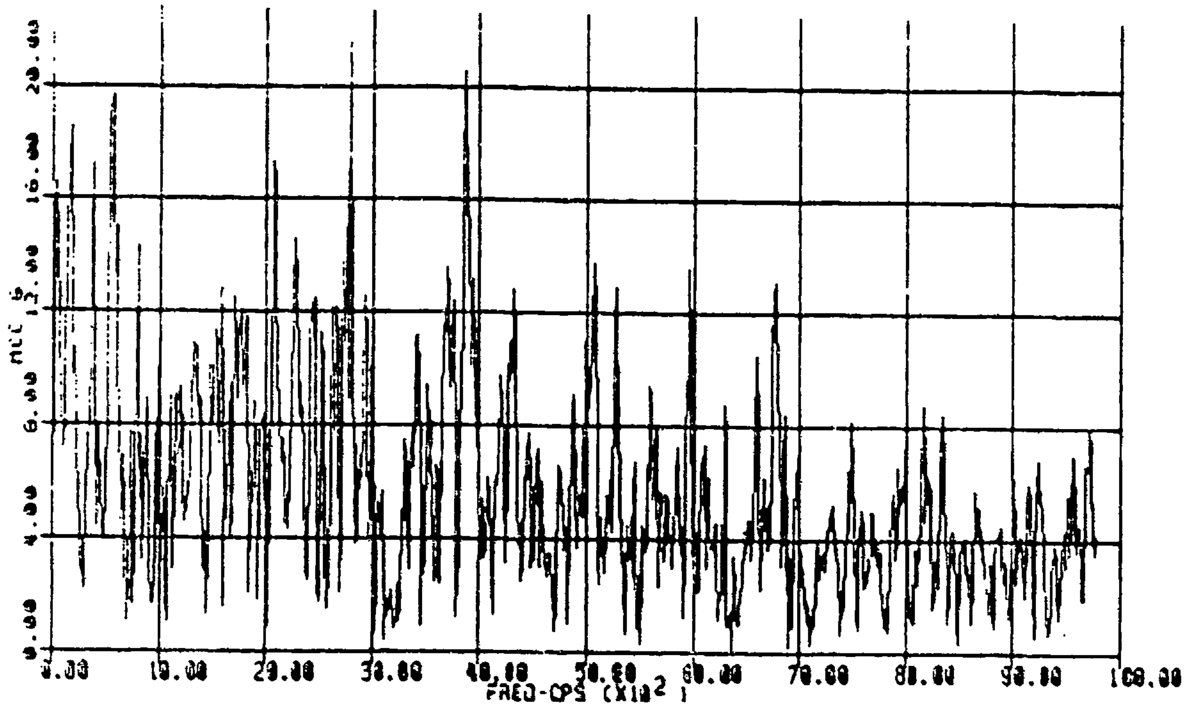

FIGURE 9(a). Fourier Magnitude

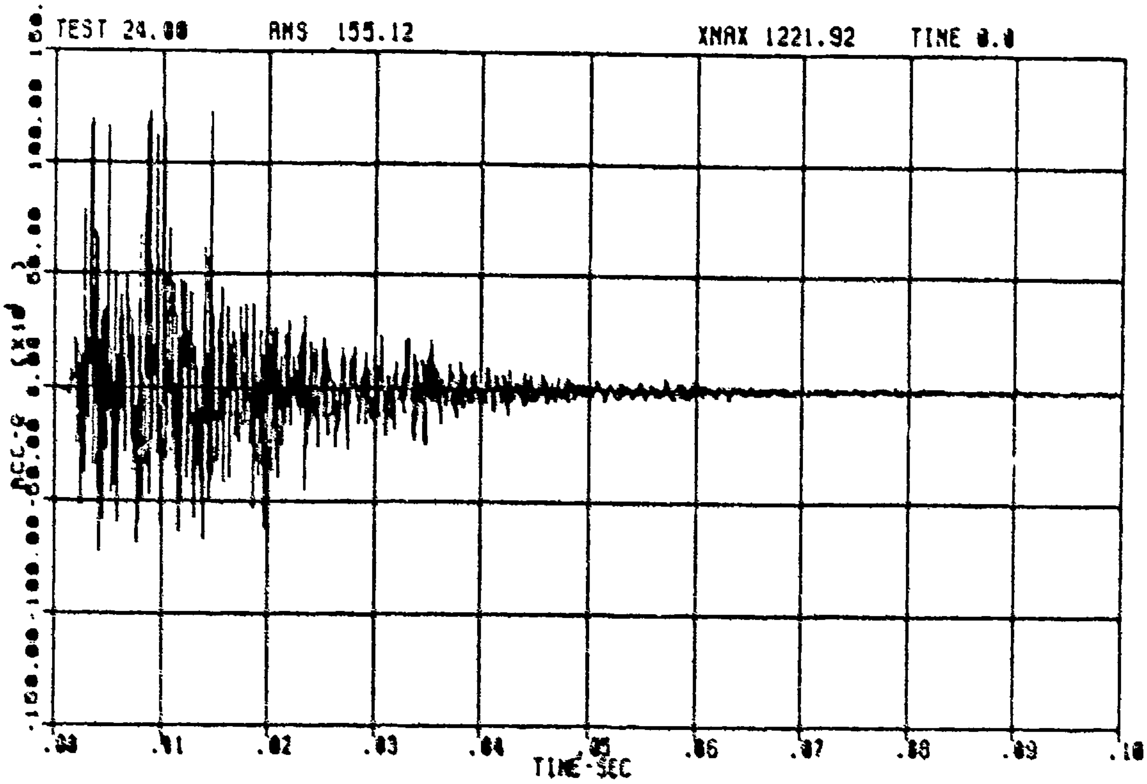

FIGURE 9(b). Acceleration-Time History

FIGURE 9. CSSI TEST SHOWING SIMILARITY TO PYROTECHNIC SHOCK (Input of seven 1 msec force pulses at 1500 lbf each, equipment acceleration 1221 g max, 155 g rms, 40 msec duration)

Discussion

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SUMMARY OF TESTING TECHNIQUES

Dan Powers
McDonnell Douglas Astronautics
Huntington Beach, CA

I have tried to survey some of the approaches that have been used to simulate pyrotechnic shock over the last 20 years. I have already learned of a few new ones today. I am going to be talking basically about two approaches: (1) the use of flight structures or flight-like structures and (2) general-purpose machines that can be used to simulate a pyrotechnic shock environment that was generated from a wide range of vehicles.

One question is often asked–why don't we just use the device itself to provide the shock? If we use the device itself we are producing a flight environment, not a qualification level that would normally be higher. To compensate for this lower level, we could perform a flight-level test three time to gain confidence. Some people in the past have fired the flight device three times in a flight structure and qualified the hardware that way. If the flight structure and an inexpensive ordnance device, such as a "pin puller" or a separation nut or bolt, are available, this may be a valid approach. However, what happens when you have a stage separation and the structure itself costs $50,000 or $100,000? After you blow it apart, only a flight environment has been produced with no qualification margin. This is part of the problem with using the device itself.

Art Ikola from Lockheed developed a concept many years ago, and he called it the "Barrel Tester." We at McDonnell Douglas read his paper and we designed a different barrel tester. Figure 1 shows the equipment compartment MDAC used. The equipment compartment separated from a Gemini spacecraft with two strips of flexible linear shaped charge cutting 0.09-inch-thick material–the flight separation joint is shown in Figure 2. High-magnitude shock was transmitted into the unpressurized compartment in which all of the electronics were mounted. Figure 3 shows the change we made so the apparatus would be reusable and attain a 6-dB qualification margin. We replaced the flight joint and left the flight-like, unpressurized compartment under it. We used a very rigid backup block, cut a groove in it, and put flexible linear shaped charge of various grain sizes in the groove. We changed the separation sheet thickness and varied that until we attained the needed 6-dB margin on the pressurized compartment. We would then mount the part at its actual flight location and fire the charge.

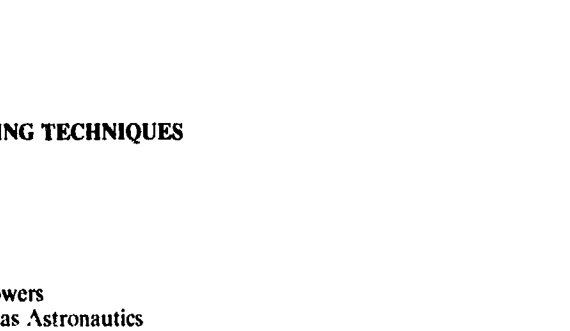

Figure 1. Equipment Bay Separated From Gemini B

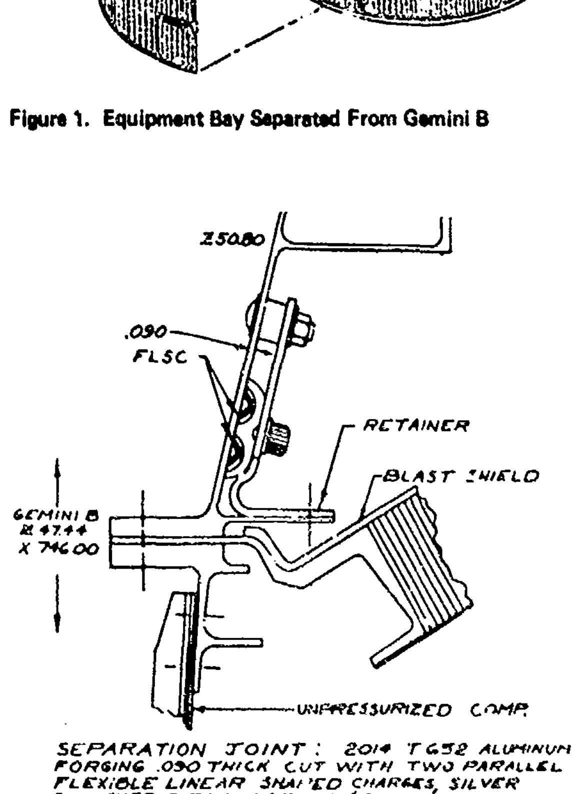

Figure 2. Flight Joint

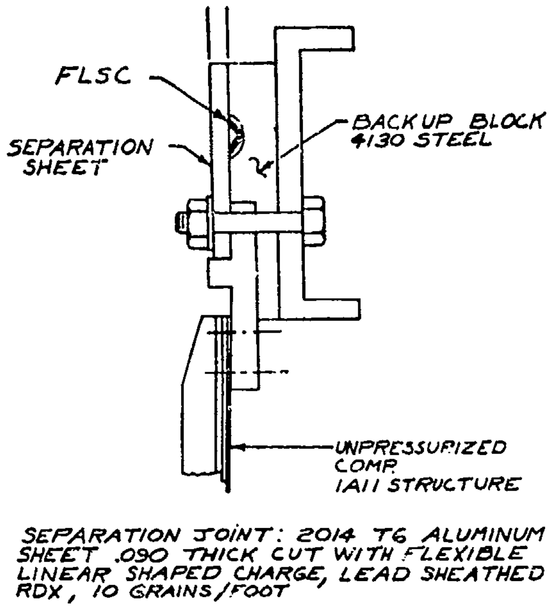

Figure 3. Barrel Testor Joint

Figure 4 shows another concept for using flight-like or flight spacecraft. I took this directly from Stan Barrett's (Martin-Marietta) paper; it shows the appearance of the Viking Lander. The central bay housed all of the electronics Figure 5 shows the bay that Stan used for the test bed. He listed numerous ordnance devices in his paper and the corresponding shock response spectra. He placed an ordnance charge at the "pyro source" and by varying the quantity could obtain margin over flight devices.

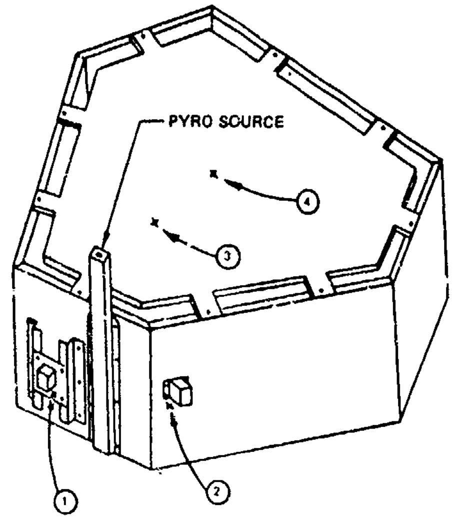

Figure 5. Viking Landing - Electronics Bay

Another group at Martin-Marietta produced a device they called "Flower Pots". The "Flower Pot" was a piece of 3-in.-diameter steel pipe with a 2-in. inside diameter 4 in. long with a 0.5-in. steel base plate welded to the bottom of it. The "Flower Pot" was mounted at the location from which the pyrotechnic shock source came. The desired spectrum was attained by varying the charge size inside the "Flower Pot". As with the Barrel Tester, they mounted the component in the actual flight location and with the increase in charge size it enabled them to get a 6-dB margin. On the same program JPL used pneumatically activated pistons to impact an anvil and generate the required shocks.

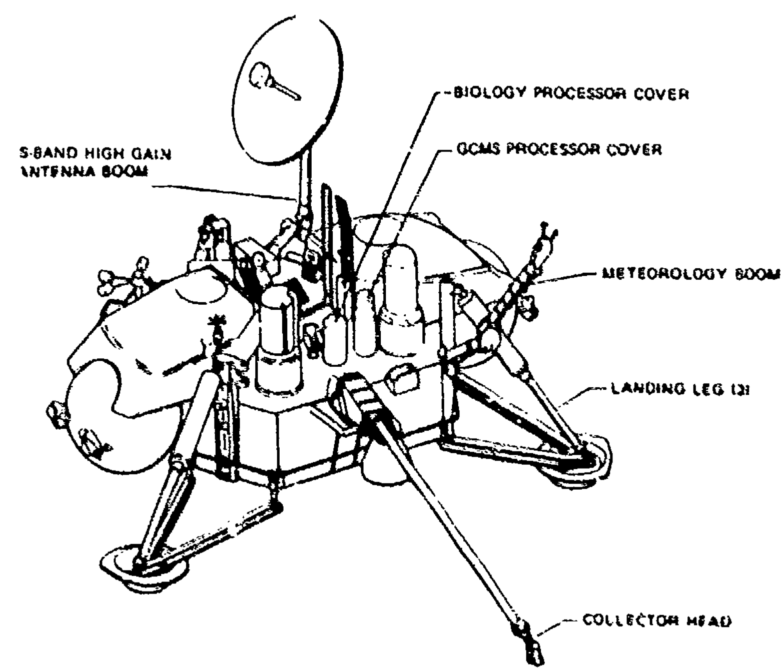

Figure 4. Viking Lander Configuration

Figure 6 shows TRW's approach. It is somewhat like Bob Morse's resonant plate. It is just an anvil on the flight-like structure. A slide hammer is raised to various heights and impacts on a fitting where the flight ordnance device is normally installed. They were able to achieve a 6-dB margin at the various flight locations.

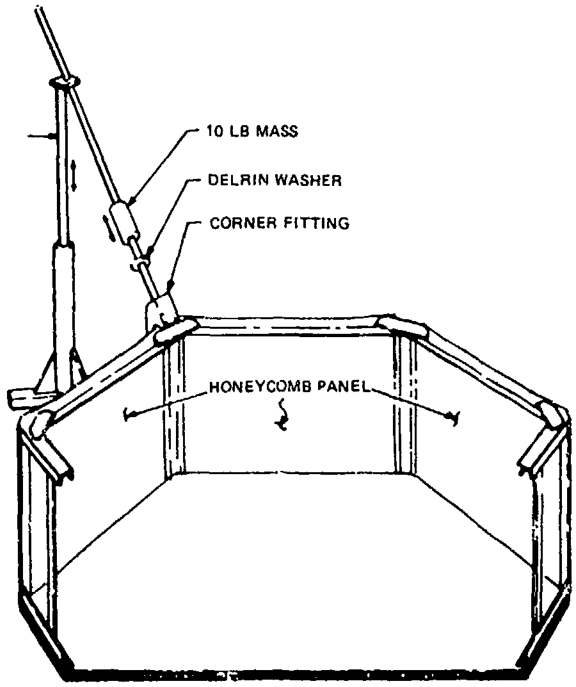

Figure 6. TRW - Shock Simulator

With the previously described apparatus you generate the right shock environment, but you generate it only for a certain vehicle because the flight-like structure responds only for that particular vehicle. It was not practical to build a shock machine for each vehicle. We wanted to build an apparatus that was more generally useful so we utilized the concept of the "Barrel Tester" joint. We took a flat piece of steel, 8 ft long, 0.5 in. thick, and 4 ft wide and put a separation joint on each end. The V joints provided a means to change the flexible linear shaped charge size. In this way we could reach 100 grains per foot on one end and as low as 10 grains per foot on the other end with various thickness sheets. Figure 7 is a photograph of the joint. The flexible linear shaped charge fits in the V-cavity. The separation sheet thickness and charge size is varied to produce the desired spectrum. At that time (1972), we did not have the intelligence to really do what Neil Davie (Sandia) has just presented and the problem as I mentioned earlier, was that we could vary the magnitude of the shock response spectrum, but were restricted to the resonance frequency of the plate, i.e., we could not change the shape of the spectrum, only the amplitude.

Figure 8 shows the test setup for a gyro package. Triaxial blocks were mounted diagonally on opposite corners and an envelope of the maxi-max response spectra was generated. Figure 9 shows that the minimum requirement was met but

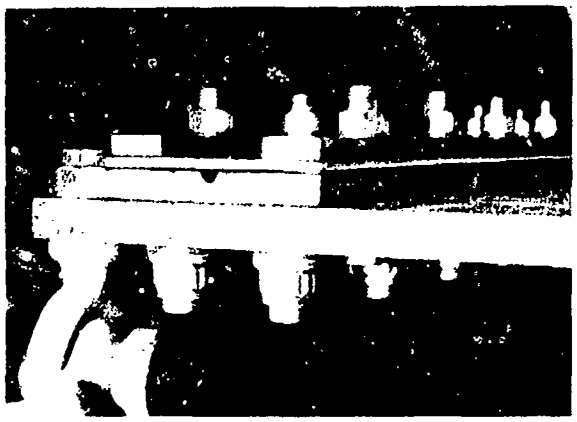

Figure 7. Flat Plate Joint

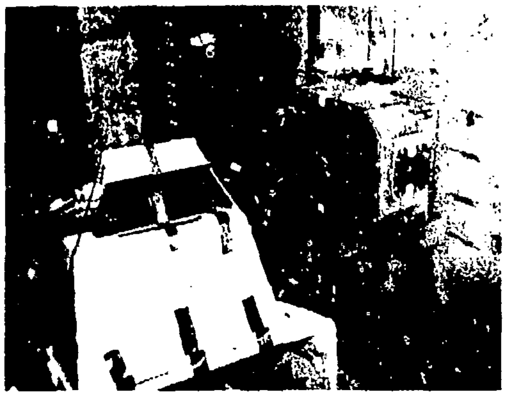

Figure 8. Gyro Package on Flat Plate

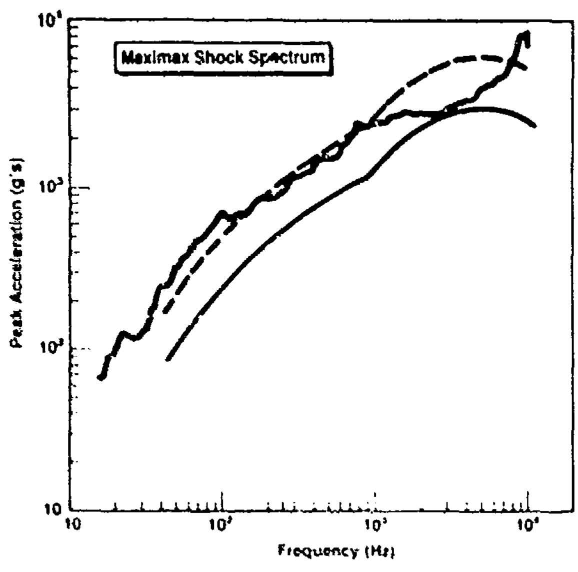

Figure 9. Gyro Package Specification

the 6-dB tolerance was exceeded at the low end. This is really the envelope of the maxi-max shock response spectrum from all three axes.

We found in the past, a compression wave comes through the plate as the explosive charge goes off. This wave was of sufficient magnitude to break off standard 10-32 accelero-

meter mounting studs. Figure 10 is the approach we use to keep our accelerometers on. We went to a 1/4 - 28 thread and bolted it all the way through the plate and then we mounted the accelerometer directly to that solid stud.

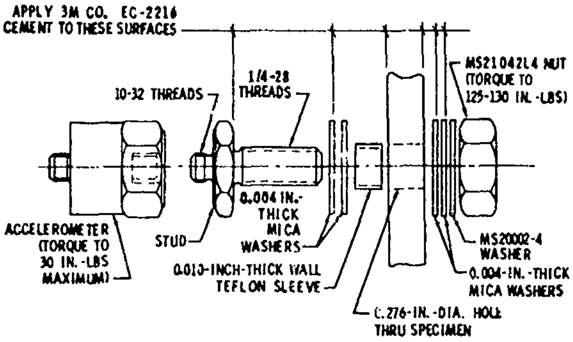

Figure 10. Accelerometer Mounting

Figure 11 shows a concept that TRW uses for simulating pyrotechnic shocks. It is a strain energy machine. The test article is mounted on top of a large block. Damping pads are on the side of the block, and a metal coupon is attached to the block. A hydraulic cylinder is pressurized until the coupon fractures. When the coupon fractures, a large amount of strain energy is released, it travels through the block, the block resonates, and the test article is subjected to a high-level transient. The main problem with this machine is shaping the spectrum.

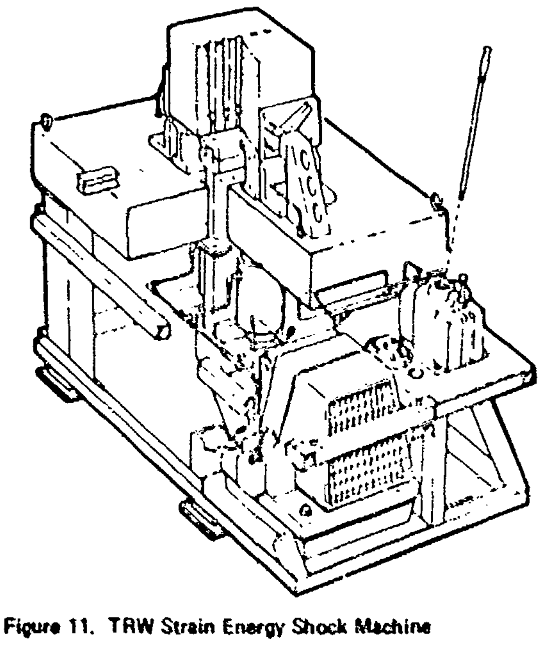

Figure 11. TRW Strain Energy Shock Machine

Figure 12 shows a rather unusual concept for simulating pyrotechnic shock. Richard Snell from McDonnell Douglas has used it in his fracture mechanics work. He has mounted some accelerometers and some strain gages on photoelastic samples. A large capacitive discharge bank can produce 300,000 g's in periods of 2 μsec. It has a Rogowski coil, and when the capacitor bank is discharged, it sends two plates together

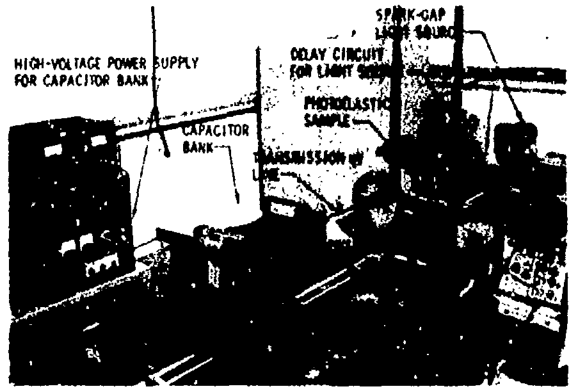

Figure 12. Stress Wave Generator

to generate an extremely short and high force transient that is transmitted to the test article.

Figure 13 shows the electrodynamic shaker. The people involved in digital vibration control have new ways of programming to meet a shock spectra, but a skilled technician can still equalize a spectrum faster than any digital system I have yet seen.

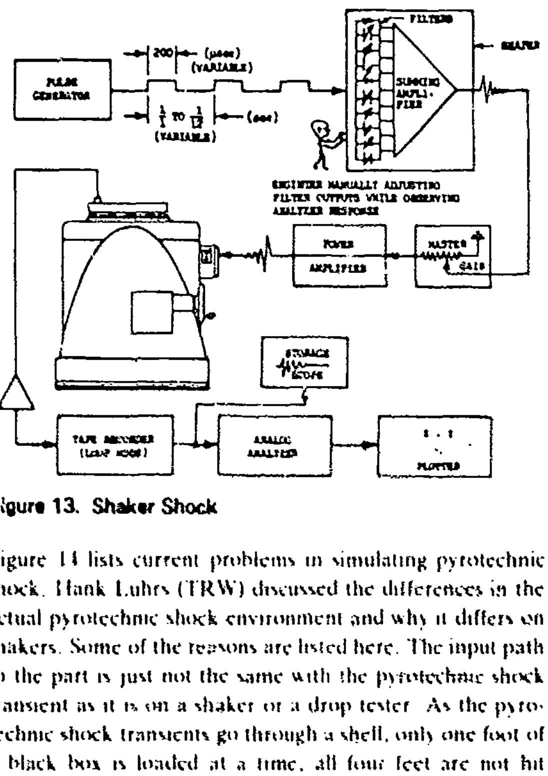

Figure 13. Shaker Shock

Figure 14 lists current problems in simulating pyrotechnic shock. Hank Luhrs (TRW) discussed the differences in the actual pyrotechnic shock environment and why it differs on shakers. Some of the reasons are listed here. The input path to the part is just not the same with the pyrotechnic shock transient as it is on a shaker or a drop tester. As the pyrotechnic shock transients go through a shell, only one foot of a black box is loaded at a time, all four feet are not hit simultaneously. The duration of the input is much shorter, maybe 10 to 15 msec in a pyrotechnic shock and typically 50 to 60 msec on a shaker. The impedance of light bracketry in flight spacecraft, where equipment is usually mounted on thin panels, is different from the impedance of a shaker where the equipment is mounted on a 100- to 400-lb armature. The velocity content of the pyrotechnic shock is much less that the shock machine. The test item absorbs less kinetic energy. The velocity of crack propagation is lower that the

1. **The input path** to the part is not the same with the pyrotechnic transient as it is with a shaker or drop tester.
2. **The duration of the input** is much shorter with the pyrotechnic transient
3. **The impedance** of the in-flight bracketry is much less than it is on any of the shock machines.
4. **The velocity content** of pyroshock is much less than that of shock machines and consequently the part absorbs less kinetic energy in a pyrotechnic shock.
5. **The velocity of crack propagation** is lower than the velocity of the extensional wave through the material; consequently, cracks that are formed do not have time to grow before the wave has passed on and the stress has been removed.
6. **For very short pulses,** fractures may occur in one area completely independent of what's happening in the rest of the part and complete failure will not occur.
7. **The ultimate strength of materials** increases significantly with increases in strain rates. It is the job of the test engineer to choose a method that will produce the same failure that would occur in the field.

Figure 14. Reasons Why Shock Simulation May Produce Different Failures Than the Actual Pyrotechnic Event

extensional wave through the material, consequently the cracks which form do not have time to grow. I showed the slide on the output of a strain gage that was located very near the source. The rate of change of strain was 2400 μin./in./sec. In Kolsky's book on solids, he shows the ultimate strength of a material can go from 50,000 to 80,000 psi when subjected to strain rates of 1000 μin./in./sec. When we are talking about pyrotechnic shock, we are definitely in this region. Now I will ask the audience to add to the list anything they think I may have missed.

Discussion

Mr. Moening (The Aerospace Corporation): Are there any advantages that you see of using an explosively driven plate over a hammer excited plate?

Mr. Powers: Let me answer that with another question. Why do transducers fail when I put them on my explosively driven plate, and why don't they fail when hit with a hammer?

Mr. Moening: I suspect the reason is that the explosively driven plate has much more of the ultra high frequency.

Mr. Powers: This is correct, actually the same thing happens, in an actual stage separation. That high frequency is there.

Mr. Moening: You are reinforcing a feeling that I had that for some limited applications that is where you have a component mounted very near the ordnance device.

Mr. Powers: That is right. If you are in area I (Neil Davie's presentation), then you have to realize that it is a different phenomenon than if you are 174 in. away and sitting on a single-degree-of-freedom system. I am talking about levels of 20,000 or 30,000 thousand g's not something 12 or 13 hundred g's. But there is a difference. If you are smart enough, you do not put electronic equipment in a 20,000-g environment even though we have qualified items to 20,000 g's.

Figures 15 and 16 show an acceleration history and its associated shock spectra near a flight separation joint. Figure 17 and 18 show comparable plots on mounting bracketry 174 in. away. The transient shown in Figure 17 certainly does not look like a pyrotechnic transient but it is. The difference is that the accelerometer is mounted far enough away from the source that it responds to the "classical" structural modes and not to the longitudinal compression and tension waves.

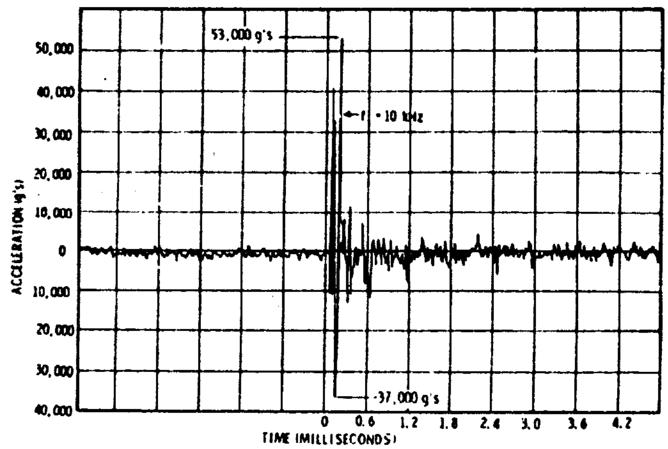

Figure 15. Acceleration History 3 Inches From Sepration Joint

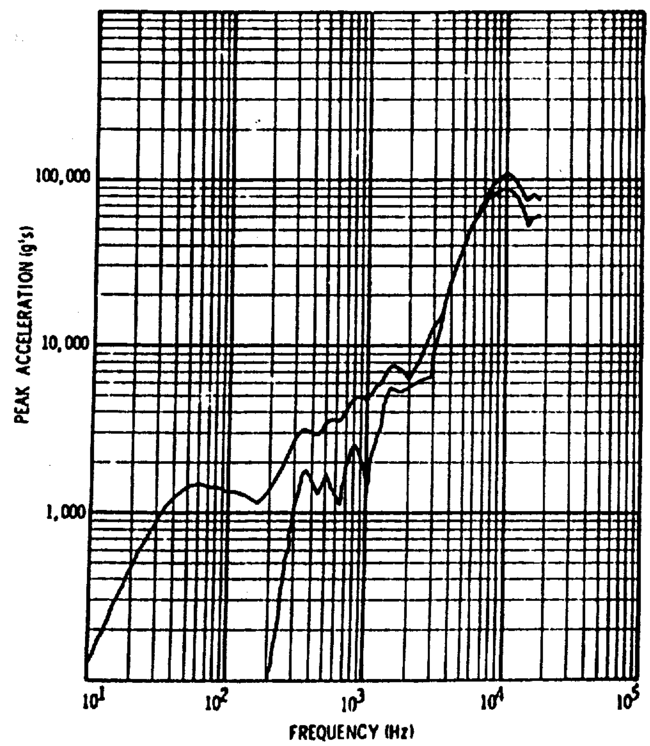

Figure 16. Shock Response Spectra Near the Separation Plane (100,000 g's at 10 kHz)

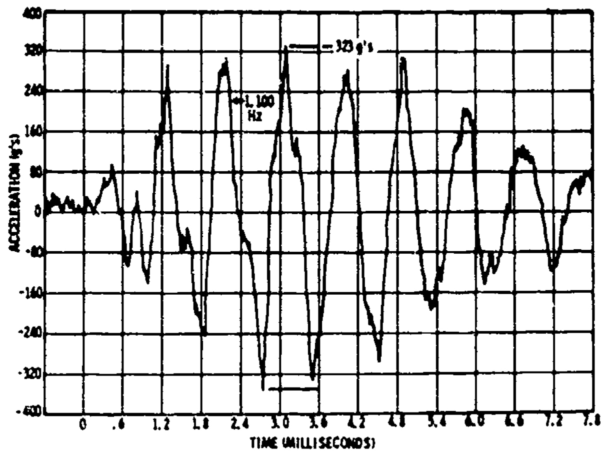

Figure 17. Acceleration History 174 Inches From Sepration Joint

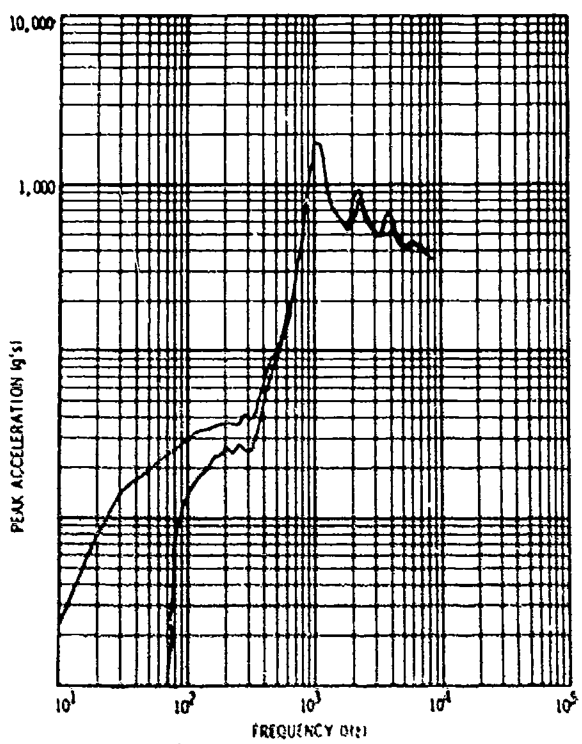

Figure 18. Shock Response Spectra 175 Inches From the Separation Plane (1800 g's at 1100 Hz)

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Mr Moening: (The Aerospace Corporation): Are there any advantages that you see of using an explosively driven plate over a hammer excited plate?

Mr. Powers: Let me answer that with another question. Why do transducers fail when I put them on my explosively driven plate, and why don't they fail if I hit it with a hammer?

Mr. Moening: I suspect the reason is that the explosively driven plate has much more of the ultra high frequency.

Mr. Powers: That is correct; actually the same thing happens, for instance, in a separation. That high frequency is there.

Mr. Moening: You are reinforcing a feeling that I had that for some limited applications that is where you have a component mounted very near the ordnance device.

Mr. Powers: That is right. If you are in area I, as Neil says then you have to realize that it is a different phenomena than if you are 174 inches away and sitting on a single degree of freedom system. Now again, I am talking about levels, of maybe 20,000 or 30,000 thousand g's not something 12 or 13 hundred Gs. But there is a difference. If you are smart enough, you do not put electronic equipment in a 20,000 g's environment even though we have qualified stuff to 20,000 g's.

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